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An Inquiry into the Functions of the Spleen, Liver, Pancreas, and Thyroid gland. By BENJAMIN RUSH, M. D. *Profeſſor of the Inſtitutes and Practice of Medicine in the Univerſity of Pennſylvania.*

OF THE FUNCTION OF THE SPLEEN.

FOUR uſes have been aſcribed to the ſpleen. It has been ſaid to prepare the blood for the ſecretion of bile from it by the liver; to be the organ in which the red globules of the blood are formed; to be a counterweight to that of the liver, on the left ſide; and laſtly, to afford an occaſional ſupply of blood to the veſſels of the ſtomach which ſecrete the gaſtric liquor, when that viſcus is unduly diſtended with food. If the uſe I am about to aſcribe to the ſpleen be admitted, it will be unneceſſary to ſtate objections to the opinions which have been mentioned.

I ſhall begin this inquiry by delivering the following propoſition.

All the motions which go forward in the human body are produced by external, and internal ſtimuli. Theſe ſtimuli exert their influence directly, or indirectly upon the blood-veſſels. From innumerable cauſes, they are liable to become *exceſſive* in their force. Such is the exceſs of this force, and ſuch the frequency of its occurrence from exerciſe, labour, intemperance, paſſions of the mind, and diſeaſe, that a proviſion to defend the tender and vital parts of the body from the effects of this force, ſeems to be a neceſſary appendage to the body. This proviſion, I believe, to be the SPLEEN. My reaſons for this opinion are founded upon the following facts.

1. The *ſtructure* of the ſpleen. It contains but one artery, and that a large one, with veins that ramify through every part of it. This artery is larger than that of the liver, which is four times as large as the ſpleen. It is, moreover, ſtronger according to Dr. Wintringham, than the aorta, from which it is derived, in the ratio of 1212, to 1000. Its veins poſſeſs a peculiar and ſpecific ſtrength, which aſſiſts in performing the office I have aſſigned to it. In having but one artery, it differs from thoſe viſcera, which have certain offices to perform upon the blood. Theſe viſcera are the lungs, the liver, and the heart, each of which has an artery intended for the excluſive purpoſe of its nouriſhment. The ſpleen having no work to perform upon the blood, and ſerving no other purpoſe, than a temporary reſervoir for it, is nouriſhed by its ſingle artery. Another artery would have been ſuperfluous to it. It has, moreover, no excretory duct. Its texture is ſoft and ſpongy, and of ſo diſtenſible a nature, that it will admit of an increaſe in its weight from blood, to three or four pounds, according to Dr. Bailie, without diſcovering the leaſt mark of a departure from its natural ſtate, in which it contains about a pound of

blood.* It is from the facility with which it is diſtended, that it has been compared to the corpora cavernoſa penis.

2. The *ſituation* of the ſpleen. It is placed near the heart, the centre and prime mover of inordinate and violent motions in the blood-veſſels, and in a part of the belly, in which from the frequent and lax ſtate of the ſtomach and bowels, and from the diſproportionate ſpace it occupies, to its ſize, it is capable of more prompt and greater diſtention, than it could have had in any other part of the body. To enable it always to retain its diſtenſible power, it is never leſſened in its ſize by fat.

3. The phenomena which take place in ſeveral of the common exerciſes of life. Where is the ſchool-boy who after running for fifteen or twenty minutes, in the ordinary plays of ſchools, has not felt a pain in the left ſide? This is ſo great ſometimes as to compel him to ſit down half bent, and even to make him cry out for relief. The name of this pain, I well recollect, indicated its cauſe. It was the "ſpleen." In laughing, the ſpleen performs the ſame kind office of opening a waſte gate, for the torrent of blood excited into action, by the violent and exceſſive agitation of the blood-veſſels; hence we ſo often obſerve perſons in paroxyſms of laughter, preſs upon the left ſide, to leſſen the pain produced by the diſtention of the ſpleen, and hence too, the phraſe of "ſplitting the ſides with laughing." It is the left ſide only, that is in danger of burſting, and the ſpleen only, in this ſide. When ſudden death occurs from laughing, it is probably induced by a rupture of the ſpleen, an accident which has, now and then, diſſections teach us, occurred from other cauſes. The pain in the left ſide, which is felt in riding a hard-trotting horſe, appears like that which is induc-

* In an account of a diſſection, communicated to Dr. Duncan, by Mr. James Elliot, he ſays "the Spleen was found, when taken from the body, to weigh eleven pounds thirteen ounces," and that its texture, figure, and bluiſh colour, were little, if at all changed." *Medical Commentaries.*

ed by running, and laughing, to ariſe wholly from an undue diſtention of the ſpleen. In all theſe caſes, it performs the office of a baſon, held by the hand of nature to receive, for a while, ſeveral pounds of blood, in order to preſerve the ſyſtem from diſeaſe and death. It is only when the ſpleen is diſtended to the extent of its capacity of retaining blood, that it imparts a ſenſe of pain, for I ſhall ſay preſently, it poſſeſſes but little ſenſibility in proportion to the number of its nerves.

4. The *quality* of the blood which has been procured from it. It is leſs coagulable than blood obtained from the arm by bleeding, or diſcharged from wounds. I have aſcribed this to the force with which it is thrown into the ſpleen, or to the feeble action of the veins, in propelling it back again into the circulation, both of which we know impair the coagulable quality in the blood. I am aware this quality in the blood is denied by Dr. Saunders, but he oppoſes to it only a ſingle experiment, made under circumſtances which do not contradict the numerous experiments of ſeveral other, equally reſpectable phyſiologiſts.

5. The nature of the diſeaſes which produce obſtructions in the ſpleen. Theſe are fevers of all kinds. Dr. Jackſon ſays, that in all the ſoldiers whom he opened, who had died of the yellow fever at St. Domingo, he found the "ſpleen full, and ready to burſt, or lax, and filled with grumous blood." In nearly an hundred perſons who died of the bilious tertians of Minorca, whoſe bodies Dr. Cleghorn examined after death, he found the ſpleen large, ſometimes weighing four or five pounds, and ſo exceſſively ſoft and rotten, that it had more the appearance of congealed blood, wrapped up in a membrane, than of an organical part." The ſame excellent phyſician tells us, that in a number of perſons who died of the dyſentery, he found the ſpleen in moſt of them "more or leſs in a putrid condition," induced probably in both caſes, by the great force, or quantity

of blood thrown into it by thoſe two diſeaſes. Morgagni ſays, "an enlargement, and obſtructions in the ſpleen are generally found in perſons who die of chronic fevers." The excitement of the blood-veſſels by the undue exerciſes of the faculties of the mind, produces the ſame morbid affections of the ſpleen. This has been proved by Dr. Proſt, who found it diſeaſed in ſeventeen out of eighteen maniacs, whoſe bodies he examined after death. The effects of the malignant paſſions, in a more eſpecial manner in diſordering the ſpleen, are admitted in common converſation, by our calling a *malicious*, a *ſplenetic* man. It has been remarked, that in perſons who die ſuddenly, the ſpleen is found to be of its natural ſize. The ſudden deſtruction of the excitement of the blood-veſſels in theſe caſes, does not give the ſpleen time enough to open its friendly door, to receive the exceſs of the ſtimulus which thus deſtroys life, by abſtracting from them for a while, three or four pounds of blood.

6. The diſeaſe which moſt commonly follows an enlarged, or obſtructed ſpleen, and that is hemorrhage. Hippocrates long ago aſcribed a bleeding from the noſe, to an obſtruction of the ſpleen. Vanſwieten ſays, he once attended a patient, in whom an uneaſineſs and tenſion of the ſpleen, enabled him to predict a return of the ſame diſeaſe. Lieutaud mentions nine caſes of perſons dying with hemorrhagies, in whom the ſpleen was found after death, diſeaſed from enlargement, and what he calls putrefaction. Four of thoſe hemorrhagies were from the noſe, the others were the ſtomach, bowels, and hemorrhoidal veſſels. There are few phyſicians that cannot ſubſcribe to the connection between hemorrhagies and obſtructions of the ſpleen. I think I have obſerved them moſt frequently to occur from the liver and ſtomach.

7thly and laſtly. The diſeaſes which follow the loſs of the ſpleen, whether by accident or deſign, in men, and other ani-

mals. Theſe are an enlargement of the liver, flatulency, indigeſtion, head-ach, and an increaſed ſecretion of ſaliva, urine, and ſemen.

I might here take notice of the exiſtence of a ſpleen, in all claſſes of animals, as a proof of its being intended for the purpoſe I have mentioned. I might likewiſe point out the difference of the ſpleen in the human ſpecies, from that of brutes, conſiſting chiefly in a texture calculated to afford more promptly and freely, a receptacle for the blood, when exerted into tumultuous motions; but this would lead me from the ſubject of the preſent inquiry. I have ſuppoſed this difference, ſo favourable to the quick diſtention and capacity of the ſpleen in the human ſpecies, to have been neceſſary, in conſequence of our blood-veſſels being more expoſed to exceſſive motions, than thoſe of inferior animals, through the medium of our greater portion of mind.

How far the ſpleen may act by abſorbing and ſuffocating undue impreſſions upon the nervous ſyſtem and the mind, I know not; but I think it probable, it ſerves this important uſe in the animal economy. My reaſons for this opinion are founded upon the great number of nerves which belong to the ſpleen, compared with its ſize, and upon its feeble ſenſibility. It is ſeldom inflamed, and when it is, the inflammation is attended with but little pain. Even wounds of the ſpleen are painful in a very feeble degree.

Upon taking a view of the manner in which other parts of the body are guarded from the evils of exceſs in quantity and motion, we ſhall be more readily induced to admit the facts and reaſonings, in favour of the uſe of the ſpleen, which I have mentioned. The eye is kindly defended from the evils of redundant light, by its black pigment; the liver from redundant bile, by a gall-bladder; the blood from redundant oil, by cells; and the

numerous cavities of the body, from redundant lymph, by a ſyſtem of abſorbents, happily calculated for that purpoſe. Why ſhould not the blood-veſſels poſſeſs a ſimilar advantage to protect themſelves from deſtruction, by means of a temporary reſervoir of their redundant motions, or quantity of blood? They would have been imperfect without it; and were it poſſible for the brain, the liver, the ſtomach, and the bowels to expreſs an opinion upon this ſubject, I believe they would ſay, they owed the preſervation of their blood-veſſels from rupture and obſtructions, and of their nerves from diſeaſe in a thouſand inſtances in the courſe of an ordinary life, to the prompt and friendly offices of the ſpleen; and were it poſſible for this long-degraded, and ever-inſulted viſcus, to obey the call to inanimate nature to praiſe its Creator, I believe it would raiſe as high a note in honor of his wiſdom and goodneſs as the eye, or the ear, or any other noble and obviouſly uſeful part of the human body.

I ſhall diſmiſs this phyſiological view of the function of the ſpleen, by briefly hinting at its application to pathology, and the practice of phyſic.

1. We are taught from the uſe that has been aſſigned to it, the neceſſity of blood-letting in all thoſe diſeaſes in which it has been found to be unduly diſtended with blood. Death is probably, in theſe caſes, the effect of an inability in the ſpleen to afford a reſervoir for a quantity of blood, ſufficient to leſſen the inordinate motions excited by it in the ſyſtem.

2. We are led by the opinion that has been delivered, to ſuſpect obſtructions to exiſt in the ſpleen, in all habitual hemorrhages, more eſpecially when they occur from the ſtomach, liver and noſe, and to rely not leſs upon aſtringent, than deobſtruent medicines, in our attempts to cure them. I am happy to find there are precedents in favour of this practice.

Donatus and Vogel, mention caſes of a vomiting of blood, being cured by removing obſtructions of the ſpleen.

3. The uſe that has been aſſigned to this viſcus, ſhould lead us to ſuſpect that many of the diſeaſes of the liver, ſtomach, lungs, and brain, are the effects of obſtructions in the ſpleen, and to employ ſuitable remedies to remove them. I am the more diſpoſed to inculcate this ſuſpicion from a fact related by Dr. Proſt, in his excellent work, entitled "Médecine eclairée par l'obſervation, et l'ouverture des corps." He ſays, of thirty-eight perſons who died of pulmonary conſumption, he found the ſpleen enlarged from two to ſix times its natural ſize, in one half of them. This is probably often the caſe when that diſeaſe is attended with a ſpitting of blood. It is poſſible, the efficacy of a ſalivation in curing it, may depend in part, upon its reſtoring the natural and healthy function of the ſpleen.

Of the Function of the Liver.

The deſign of the liver, I believe to be, to receive the blood from every part of the body, in order to ſubject that part of it, which had not been completely animaliſed, or diveſted of its chylous properties, to a ſecretory proceſs, and afterwards to pour the product of this ſecretion, mixed with the liquor of the pancreas, into the duodenum, to be abſorbed, or otherwiſe taken up by the lacteals, and conveyed with the chyle from the ſtomach into the blood-veſſels, in order to be completely converted into red blood, for the purpoſe of ſerving the various and important uſes, for which that fluid is intended in the human body.

The facts which have led me to adopt this opinion, are as follow.

1. The liver is preſent in nearly all animals. In this reſpect, it is upon a footing with the ſtomach. Of courſe there is reaſon to ſuppoſe it is deſigned to perform an office in the ſyſtem, equally neceſſary with the ſtomach, to the ſupport of life. It is no objection to the truth of this remark, that in ſome animals, there is no gall-bladder attached to the liver, and that in others, it diſcharges its contents into the ſtomach, or the bowels remote from the liver; for I hope to ſhew preſently, that the cyſtic and hepatic bile, ſerve very different purpoſes in the animal economy.

2. The immenſe and diſproportionate ſize of the liver, in the fœtus, compared with its ſize in adults, the deſign of which appears to be, that nouriſhment may be carried on excluſively by that viſcus, without any aid from the ſtomach.

3. The ſize of the liver in adults, and the quantity of bile which is ſecreted, ſuppoſed by Dr. Haller, to be four and twenty ounces, in four and twenty hours; five-ſixths of which he ſuppoſes, paſs directly into the duodenum. It is not probable this large apparatus, and copious ſecretion, can be of an excrementitious nature, and I ſhall hereafter mention ſome facts to prove, that it is not eſſential to the production of the chyle that deſcends from the ſtomach, and that it is effuſed, ſubſequent to the chyle having paſſed by the duodenum.

4. Chyle is found in the blood, after it has paſſed through the lungs. This has often been obſerved, when it has been drawn, ſoon after a full meal. It has likewiſe been demonſtrated by Dr. Hutchinſon, in his inaugural diſſertation, upon "the converſion of chyle into the blood," publiſhed in the year 1804, by a number of experiments, made upon the blood of living dogs. The chyle in theſe caſes, partakes too much of the nature of the aliments from which it is formed to be changed into

blood in the lungs, by the action of the air upon it, without undergoing a ſecond chylopoetic proceſs in the liver.

5. The quality of the venous blood, from which the hepatic bile is ſecreted. It is leſs diſpoſed to putrefaction, than arterial blood, taken from any other part of the body. This has been aſcribed by Dr. Caldwell, to its having parted with its oxygen. It is poſſible this may be one of the cauſes of its being leſs putreſcent, than arterial blood; but I am diſpoſed to aſcribe it chiefly to its containing a quantity of imperfect chyle, which we know paſſes more ſlowly into a putrid ſtate than blood. That this is the caſe, has been proved accidentally by Dr. Hutchinſon, by an experiment made without any reference to this ſubject, or even to that of his diſſertation; alſo by an experiment, made by Dr. Rollo, in which he found the blood of a man in a diabetes, to be leſs putreſcent, than the blood of a perſon in health. In that diſeaſe, we know the blood contains a preternatural quantity of chyle, often ſo great, as to diſcover itſelf, not only in the urine, but in ſeveral other of the ſecretions. Dr. Haller ſays, he has ſeen fat in the vena portarum. I ſuſpect the matter he ſuppoſed to be fat, was a portion of imperfect chyle.

6. The quality of the hepatic bile. Dr. Boerhaave, who ſays, he had taſted it, aſſerts that it is "mild, ſweetiſh, and watery." Dr. Haller ſays of it, "dulcior hepatica bilis, cyſtica amara;" but he adds, that he found a conſiderable bitterneſs in it, in a man who had been hung, and in a woman who had died ſuddenly. In both caſes, this bitterneſs was probably imparted to it in the act of dying, by a mixture of cyſtic bile with it. In animals, which have no gall-bladder, the Doctor admits the hepatic bile to be "uniformly ſweet." It is certain the livers of thoſe animals, which form a part of our aliment, have not the leaſt bitter taſte, except in thoſe caſes in which the contents of the gall-bladder have accidentally fallen upon them, in

the act of dreſſing them for the table. The hepatic bile is always ſweet in new-born infants. By a chemical analyſis, even the cyſtic bile of an adult, yields a portion of albumen, which we know to be one of the component parts of the chyle.

7. There are ſeveral experiments related by Dr. Fordyce, which prove chyle to be formed by the action of the ſaliva and gaſtric juice upon the aliment, without the mixture of hepatic bile with it. The Doctor tied up the ductus communis of an animal, and found the chyle afterwards to poſſeſs its uſual healthy properties. The ſame healthy and natural ſtate of the chyle, has been obſerved in thoſe caſes, when, not only the excretion, but the ſecretion of bile has been ſuſpended by that torpid ſtate of the liver, which has lately received from Dr. Pierſon, the appropriate name of Hepatalgia.

8. The ſtructure, ſituation, and function of the PANCREAS. It reſembles the ſalivary glands in its ſtructure; it ſecretes a liquor, which poſſeſſes the ſame diſſolving and animalizing properties as the ſaliva, and it pours this liquor ſo directly upon the hepatic bile, in the common duct, before it enters the duodenum, as to act upon it in a concentrated ſtate, and thereby to change it into perfect chyle. By aſcribing this uſe to the Pancreas, we reſcue it from its inſignificant office of performing a work of ſupererogation only to the ſaliva, and gaſtric juice, and give it a little ſovereignty or independent juriſdiction in the animal economy.

9thly and laſtly. I infer, that a ſecond chylopoetic proceſs goes forward in the liver, from the effects of intemperance upon it. It increaſes its labour, and thereby increaſes its ſize. This has often been obſerved in full feeders. But it does more. It produces a preternatural ſecretion of bile, more eſpecially when aliments conſiſt of an undue proportion of fat ſubſtances, which reſiſt the powers of digeſtion in the ſtomach. When

this labour has been long impoſed upon the liver, we obſerve it to ſuccumb like the ſtomach under hard uſage, and to produce, with its diſeaſes, the ſame morbid affections in other parts of the body.

Let us next inquire into the uſe of the gall-bladder, and the cyſtic bile.

From the ſituation of the gall-bladder, from the acute angle its duct makes with the hepatic duct, where they form the ductus communis, and from the circumſtances which influence its fulneſs, and depletion, I believe it to be intended wholly to ſerve the ſame purpoſe with reſpect to the liver, which the ſpleen ſerves to the whole ſanguiferous ſyſtem, that is, to afford a receptacle for redundant bile, and thereby to prevent the obſtruction of the hepatic bile into the duodenum, and its regurgitation into the pori biliarii. Unleſs this proviſion had been made for an exceſs in the ſecretion of bile, to which ſo many accidental cauſes contribute, the liver would have been expoſed to diſeaſe and diſorganization every day. It is poſſible, the leſs nutritious particles of the hepatic bile may be thrown into the gall-bladder, but whether this be the caſe, or not, the bile appears to undergo a putrefactive proceſs in it. I infer this, from the bitterneſs it acquires during its ſtagnation in the gall-bladder. The ſame bitter quality, is the offspring of the putrefaction of certain vegetables, and of ſome animal ſubſtances. It probably takes place in other ſecretions of the body. Dr. Darwin aſcribes the bitter taſte we often perceive upon the tongue, to a morbid change in the quality of the liquor, which is ſecreted upon its ſurface; and perhaps the bitterneſs of the wax, which is ſecreted in the meatus auditorius is derived from a change induced in it, by its ſtagnation, ſimilar to that which occurs in the bile. It is true, the bile is leſs putreſcent than the blood, according to an experiment made by Dr. Saunders, and ſince repeated by a graduate, in the Univerſity of Pennſyl-

vania; but the ſame indiſpoſition to putrefaction takes place in certain vegetables, after they acquire, by decay, a bitter taſte. Of the truth of this remark, I ſatisfied myſelf by the following experiment. Into a two-ounce phial, I put half an ounce of beef, cut into ſmall pieces, with the ſame quantity of the rotten and bitter part of an apple, and an ounce of water. Into another phial, I put the ſame quantity of beef and water, and placed them both under the ſame circumſtances of heat. The latter putrefied many hours before the former. Had the rotten part of the apple been expoſed alone to heat and air; I have no doubt it would have undergone another putrefaction. The ſame remark applies to the bile, out of the body. Even the fæces undergo a ſimilar change, and hence we find them inoffenſive, as remote cauſes of diſeaſe, unleſs they have been expoſed long enough to the action of the ſun and air, to excite in them a ſecond putrefaction. While I ſuppoſe the bile to be of an excrementitious nature, I admit that it ſerves ſeveral valuable purpoſes in the animal economy. In ſtimulating the bowels, it enables them to propel their contents downwards, and imparts to them at the ſame time a tone, which is communicated to the whole ſyſtem. It ſerves likewiſe, to precipitate thoſe parts which are incapable of affording nouriſhment to the body from the chyle; and perhaps, it retards its tendency to putrefaction, eſpecially in hot weather. Should the bile exert this antiſeptic quality, it will only be upon a footing with the nitrate of potaſs, and ammonia; both of which, although the offspring of putrefaction, have a powerful effect in preventing it.

The liver has been called, by ſome phyſiologiſts, a ſecretory, and by others, an excretory organ. The reader will perceive, that I adopt both theſe opinions. The hepatic duct diſcharges its ſecretion, and the cyſtic duct its excretion into the duodenum. The gall-bladder appears to be to the liver, what the colon and rectum are to the ſtomach; the receptacle only of hepatic

fæces. The quantities of each, accord with the exceſs or deficiency of aliment taken into one, and of imperfect chyle received and ſecreted by the other.

From a review of the function of the liver, which has been delivered, we are led, in the firſt place, to admire the goodneſs of the Creator of our bodies, in thus providing two viſcera for preparing the matter, which is neceſſary to repair their daily waſte, each of which is provided with an ample apparatus for that purpoſe. The nouriſhment of the body is, in this reſpect, upon a footing with the double ſenſes of viſion and hearing. In caſes of ſickneſs, indigeſtion, or long faſting, in which the office of the ſtomach is ſuſpended, the liver performs a vicarious duty; and when its chylopoetic office is impaired, or interrupted, the ſtomach, by performing its functions with double care, prevents the evils of an abſtraction of nouriſhment from the body. But there is another inſtance of alternation between the ſtomach and liver, in their reſpective offices. While the former is moſt buſy, the latter, from the preſſure of the ſtomach upon it, is moſt idle. It is only after the chyle from the ſtomach has paſſed by the duodenum, that the liver pours its chyle into it. In this way the bowels, and the lacteals are kept in a conſtant ſtate of moderate tenſion, and activity. The ſame diſtention and preſſure of the ſtomach, which impedes the ſecretion of hepatic chyle, ſqueezes the cyſtic bile from the gall-bladder; by which means it is enabled to perform one of the offices I have aſcribed to it: namely, to ſeparate the fæcal matters from the chyle. From the ſituation of the gall-bladder in the concave part of the liver, the preſſure of the ſtomach is neceſſary to elevate its bile to the level of the cyſtic duct, in an erect poſture of the body. In a recumbent poſture, the bile deſcends more eaſily into the duodenum, eſpecially in lying upon the back. It is from its more copious effuſion into the bowels during ſleep, that the practice of going to ſtool in the morning, has been one of the habits of

the human body in all ages and countries. It is from the ſame cauſe that perſons who are afflicted with an exceſſive ſecretion, and excretion of bile, are moſt apt to be ſick, or puke bile in the morning.

I am aware that I differ from all modern and ancient phyſiologiſts, in ſuppoſing that the preſſure of the ſtomach upon the liver, ſuſpends the ſecretion of bile. I infer it, becauſe preſſure ſuſpends ſecretion in all other glands. We often ſee it in the kidneys from the preſſure of the colon, when diſtended with fæces upon them. The current opinion of the ſecretion of bile, being increaſed by the preſſure of the ſtomach upon the liver, took its riſe in a belief that its mixture with the aliment in the duodenum, was neceſſary to conſtitute chyle. The reader will perceive further, that I do not admit of the gall-bladder being filled by the regurgitation of hepatic bile, in conſequence of an obſtruction, induced upon the ductus communis, by the diſtention of the duodenum. I ſuppoſe it to be filled only when the liver ſecretes and pours its bile in too large quantities to be conveyed into the bowels. In this caſe, it affords the ſame relief to the liver, that the ſpleen affords to the blood-veſſels, by becoming its waſte-gate. Perhaps the paſſage of the bile into the gall-bladder, may be aſſiſted by the fibres of the muſcular coat of the cyſtic duct, performing their actions in that direction. In this way we know many fluids are propelled, contrary to their gravity, in different parts of the body.

2. The economy of the double proceſs which has been deſcribed for preparing the nouriſhment of the body, deſerves our notice. Had it been otherwiſe, all that portion of chyle, which was incapable of being made into blood, by paſſing through the lungs, would have been loſt to the ſyſtem. If the office I have aſſigned to the liver, be correct, it may be compared to the ſecond bolting cloth of a mill; or perhaps, more properly, a frugal houſewife, who collects all the meat which has refuſed to

yield its nouriſhment to a ſingle operation of fire; and by cooking it a ſecond time in other veſſels, and by other modes of heat, extracts from it a ſecond portion of nouriſhment.

3. From the opinion of the uſe of the liver which has been delivered, we are enabled to underſtand the reaſon, why we often obſerve large and offenſive fæcal diſcharges, in perſons who have eaten little or no food. I have frequently ſeen them with ſurpriſe in children, while labouring under the colera infantum, who had paſſed many days without retaining a ſpoonful of aliment, or even of water upon their ſtomachs. The ſame remark is made by Dr. Cleghorn, in his account of the dyſentery in Minorca. It is ſo pertinent to our ſubject that I ſhall inſert it in his own words. After mentioning the exhibition of purges, he ſays "They brought off a prodigious quantity of round hard fætid lumps, to the great relief of the patient; nor is it eaſy to conceive how ſo much had been collected, or where it had been lodged ſo long; as in ſome caſes I have obſerved, the patients having eaten nothing for two or three weeks, that could furniſh much excrement; and during that time had taken ſeveral glyſters, and common cathartics which brought away liquid ſtools." From the diſpoſition which the bile has to form itſelf into ſolid ſubſtances, or what are called gall-ſtones; I am diſpoſed to believe that the lumps taken notice of by Dr. Cleghorn, or what we call Syballa, are always compoſed of hepatic fæces. The meconium of children, appears to be formed from them.

4. The uſe which has been aſcribed to the liver, leads us to diſcover the reaſon why a diſeaſed ſtate of the ſtomach, is ſo often followed by a diſeaſed ſtate of the liver. When the former performs its work of digeſtion imperfectly, it impoſes double labour upon the latter, in preparing the chyle for the nouriſhment of the body, and this neceſſarily brings on diſeaſe.

5. It has often been remarked, that the digeſtive proceſs in the ſtomach required a healthy and tranquil ſtate of the whole ſyſtem. The ſame thing may be ſaid of the chylopoetic proceſs in the liver. It is impaired by all general, and by many local diſeaſes. It ſuffers, according to Dr. Moſely, in all the acute and chronic diſeaſes of the Weſt Indies. Dr. Paley aſcribes to it, the ſame ſympathy, with all the diſeaſes of the Eaſt-Indies. Dr. Boerhaave ſuppoſed it to be affected, in ninety-nine out of an hundred chronic diſeaſes, from all their cauſes. There is ſcarcely a diſeaſe in the head, ſo light, that does not diſturb its natural actions in a greater or leſs degree.

6. Children are often affected with a ſudden ſwelling and hardneſs of their bellies, which as ſuddenly ſubſide, from the operation of a purge, or from friction with the hand. It has been aſcribed to wind, alſo to worms in their bowels. May it not be occaſioned by a temporary engorgement of the liver, brought on by an exceſs of aliment taken into the ſtomach, and by a redundant quantity of chyle, prepared from it?

The opinion that has been delivered of the function of the liver, is calculated, not only to explain the cauſes and phænomena of ſeveral diſeaſes, but to ſuggeſt ſeveral inferences that may be uſeful in the practice of phyſic.

1. Has the liver been elevated from its former humble and tributary ſituation in the ſyſtem, to ſhare with the ſtomach, in the high and important office of preparing nouriſhment for the body? Let us never forget to inquire into its ſtate, with the ſame ſolicitude that we inquire into the ſtate of the ſtomach, in all general diſeaſes.

2. Has a ſympathy been pointed out between the ſtomach and liver in their reſpective offices? Let us ſuſpect their diſeaſes to be reciprocal. The influence of the liver, when

diſeaſed, in bringing on ſtomach complaints, is well known, and Dr. Thomas Clark has informed us in his "obſervations upon the diſeaſes of the Weſt and Eaſt Indies," that a diſeaſed ſtomach, is the conſtant, and often the firſt ſign of a diſeaſed liver in that country.

3. Do we wiſh to reduce the ſyſtem by the *ſudden* abſtraction of nouriſhment? In vain ſhall we attempt to do ſo, by limiting the quantity of aliment taken into the ſtomach, while the liver continues to ſupply the lacteals with alimentary matter. It can only be effected by combining active purges with abſtinence from food.

4. Do we often meet with a preternatural ſecretion and excretion of bile appearing in headach, languor, ſickneſs, puking of bile, and an occaſional diarrhœa? I have taken the liberty to call this diſeaſe, a diabetes of the liver, and have ſuppoſed it to depend upon original debility, and increaſed morbid action in its ſecretory and excretory veſſels. It occurs in the gout, in perſons habitually intemperate, and in the ſeaſon of bilious fevers in all warm countries. It yields to evacuating or tonic remedies, according to the ſtate of the ſyſtem.

5. Do we now and then meet with a bilious diarrhœa, attended with the appetite and digeſtion unimpaired? I have called this diſeaſe a lientery of the liver, and have ſuppoſed that the chyle paſſes through it into the bowels, without undergoing the change that is neceſſary to procure its admiſſion into the lacteals. The remedy in this diſeaſe is depletion, more eſpecially by abſtracting nouriſhing aliment. I have known it to yield to a diet conſiſting wholly of vegetables, after purges and the moſt powerful aſtringents had been uſed to no purpoſe.

6. Does the formation of ſound and healthy blood depend upon the healthy action of the liver, and does the liver ſuffer more or leſs, from all general, and many local diſeaſes? It follows, of courſe, that a medicine which is ſaid to act ſpecifically upon glandular parts of the body, and which is known to act powerfully upon the liver, occupies deſervedly the firſt rank of all the articles in the Materia Medica. Such a medicine is mercury, and hence its uſefulneſs and fame in all general and chronic diſeaſes.

7. Is the ſtomach, from diſeaſe, unable to retain or digeſt aliment, or is its paſſage into the ſtomach, or duodenum, obſtructed by ſchirrus, or any other cauſe? The uſe that has been aſſigned to the liver, ſhould lead us to double our exertions in conveying aliment into the ſyſtem, by means of injections into the lower bowels. It has been objected to this mode of nouriſhing the body, that aliment, when abſorbed, could not perform that office without being converted into chyle, and that this liquor was prepared, excluſively, by the ſtomach. We now ſee a change of aliment into chyle may be performed, independently of the ſtomach, by means of the liver.

Of the Functions of the Thyroid Gland.

The deſign of this gland, I believe, to be to defend the brain from the morbid effects of all thoſe cauſes which determine the blood into it, with unuſual force.

My reaſons for this belief are founded, 1ſt. upon its ſituation and ſtructure. It is ſeated upon the anterior parts of the larynx, and is ſupplied with four large arteries, which return their

blood by means of veins, without terminating in an excretory duct, or producing any thing like a ſecreted liquor.

2. Upon its larger ſize in women than in men. This proviſion was neceſſary to guard the female ſyſtem from the influence of the more numerous cauſes of irritation and vexation of mind, and the more acute bodily diſeaſes, to which they are expoſed, than the male ſex. The ſenſation known by the name of globus hyſtericus, appears to be produced by the diverſion of exceſſive mental impreſſions from the brain to the thyroid gland. We often obſerve it to be conſiderably enlarged, in hyſterical paroxyſms. A remarkable caſe of this kind, is taken notice of, by Dr. Whyt, in his treatiſe upon nervous diſeaſes. It is probably from the greater ſize, and more frequent excitement of the thyroid gland in women than in men, that the former are more ſubject to Bronchocele, than the latter.

3. Upon the effect of certain exerciſes of the body and mind, upon the thyroid gland in its diſeaſed ſtate. Dr. Broadbelt relates in his inaugural diſſertation upon Bronchocele, publiſhed in Edinburgh, in the year 1794, that ſuch of the inhabitants of Derbyſhire, in England, as are afflicted with that diſeaſe, are ſubject to a pain in the gland, and to an increaſe of its ſize when they are unuſually excited by running or anger.

4. Upon the effect which diſeaſe in the thyroid gland, and its loſs, have upon the brain. The bronchocele of the Cretins is generally accompanied with imbecility of mind; and Dr. Chapman informed me, that Mr. Cooper, Surgeon of St. Thomas's Hoſpital, in London, had produced ſomething like fatuity in ſeveral dogs, by extirpating this gland.

It is poſſible this gland may ſerve the additional purpoſe of an outlet, to undue impreſſions upon the lungs and windpipe, by

an exceſs in the exerciſes of the voice and ſpeech, and thereby defend thoſe important parts of the body from rupture and diſeaſe.

I ſhall conclude this inquiry, into the functions of the ſpleen, liver, pancreas, and thyroid gland, by informing the reader, that I have yielded reluctantly to the ſolicitations of my pupils, and the advice of my medical friends, in offering it to the public in its preſent immature ſtate. My beſt wiſhes are for an early examination, and a ſpeedy refutation of the opinions contained in it, if they are erroneous; and if otherwiſe, that the inferences to which they lead, may be immediately applied to the practice of medicine.*

Philadelphia, May 13th, 1806.

* Since I have adopted, and taught the uſe I have aſſigned to the ſpleen, a former pupil of mine, copied from a French edition of one of Heiſter's works, by Dr. Senac, in the poſſeſſion of a phyſician, in a neighbouring ſtate, and put into my hands a ſhort note, in which the Doctor glances at the opinion I have delivered; but rejects it as chimerical. The peculiar ideas upon that ſubject, and all the others contained in the above inquiry, occurred to me, while I was employed in inveſtigating the cauſes of the obſtructions in the ſpleen and liver, which take place in madneſs.

Reply to Dr. Peachey Harrison's *Obſervations on Impregnation.*

By William P. Dewees, M. D.

Philadelphia, April 2d, 1806.

Sir,

I AM extremely averſe to enter on any thing like controverſy; but am obliged to notice, "Obſervations on Impregnation, by Dr. Peachey Harriſon," as, the author of them ſeems ſatisfied that he has removed the obſcurity that has ſo long veiled this intereſting ſubject. This, in my opinion, he has not ſucceeded in,* notwithſtanding he has purſued the "coy maid" (Nature) into ſome of her receſſes, and endeavoured to extort from her ſome of her oracles. I boaſt of no ſuch familiarity with this high perſonage, and ſhall be very well ſatisfied if ever ſhe allows me a glimpſe of her operations, either through keyhole or cranny. I freely acknowledge ſhe has never admitted me into her receſſes; I will not therefore pretend with certainty to ſay, how ſhe performs her ſecret actions within them.

Did we implicitly rely on this gentleman's aſſertion, we ſhould have nothing to wiſh for on this ſubject, as he aſſures us his opinions are not "deduced from uncertain conjectures, but from reſponſes, rendered by interrogated nature." But alas! this ſame "coy maid" when he purſued her into her ſecret receſſes, appears to have played him a ſlippery trick; for, inſtead of "the reſponſes of interrogated nature," ſhe appears to have furniſhed him with nothing but "uncertain conjectures;" this ſhe

* As he has not brought forward a ſingle ſubſtantiated fact, a probable conjecture, or a plauſible hypotheſis.

perhaps has inflicted on him as a puniſhment for purſuing "her into her ſecret receſſes," and endeavouring to extort from her ſome of the oracles. He confeſſedly admits her reſponſes were extorted, they muſt therefore have been given unwillingly; and that which is given by "compulſion" (even, where reaſons are as plenty as blackberries) is not always given truly. But can that which is given by Nature be otherwiſe than true? No; but is the gentleman certain that it was from Nature's oracle the reſponſes came?

Let us endeavour to aſcertain this; he tells us, p. 422, "That there is a ſpecific ſenſibility reſident in the female genital ſyſtem;" and "this ſenſibility reſides in the os tincæ chiefly, but alſo in the uterus itſelf, in the fallopian tubes, and *perhaps*, in the ovaria." Is this conjecture, or the reſponſes of nature? Part is certainly admitted as ſuch, ſince he ſays, *perhaps* in the ovaria; this part certainly is not the dictum of the "coy maid." But let us proceed. "I am inclined to think, (mark, inclined to think) the ovaria poſſeſs a ſhare of venereal ſenſibility; becauſe, as venereal deſire is firſt awakened in males by the ſtimulus of the ſemen, *ſo it is probable* (very candid from a man whoſe opinions are not deduced from "uncertain conjectures") it is firſt arouſed in females by one or more ovula acquiring maturity." Are theſe facts? No; it is well known that venereal deſire is awakened in males before the ſecretion of ſemen, and after the extirpation of the teſtes. Boys at very early periods give evidence of paſſion of this kind, before we can poſſibly ſuppoſe ſemen to have been ſecreted, unleſs this ſecretion takes place at a period not dreamed of by phyſiologiſts. Who has not witneſſed the moſt flagitiouſly wanton acts in boys of ten years old or even younger? I myſelf have lately witneſſed an act of this kind, the moſt horrible to be conceived of; a boy of nine years old, was caught in the act for venereal gratification, with a child of eighteen months, the child was under my care,

and much injured. But this is by no means a ſolitary caſe; every boarding ſchool for boys, will furniſh proofs of early deſire. After the extirpation of the teſtes we know deſire remains; the eunuch, the horſe, and barrow furniſh inſtances of it. In the female nearly the ſame obſervations will apply; girls are known to exhibit many marks of libidinous deſire, before we can with propriety ſuppoſe "one or more ovula" have acquired maturity; they certainly do not have theſe deſires where we know "one or more ovula" are perfected; and women retain their venereal appetite, when we muſt ſuppoſe there is not "one or more ovula" to be matured; namely, after the menſes have ceaſed to flow; and we may add, there are women of very warm paſſions, and who perhaps have enjoyment from ſexual intercourſe in its moſt exalted form, yet ever remain barren. Where are the ripened ovula in ſuch women, or how are we to account for their receiving gratification, when we have no right to ſuppoſe, "one or more ovula" have acquired perfection to produce the venereal impulſe?

"That the venereal ſenſibility reſides in the os tincæ, &c." continues Dr. Harriſon, "I infer from the ſtructure of the parts, and eſpecially from the projection of the collum uteri into the vagina, where it will of neceſſity, in the ſexual intercourſe, receive irritation from the ſoft and velvet-like head of the penis, well calculated to produce what I have called the venereal orgaſm; which conſiſts in a certain excitement of the uterine ſyſtem, accompanied with exquiſite ſenſations, ſimilar to thoſe which take place in the male, at the time of the ejection of the ſemen, and, with *an unknown deſire* to receive the venereal ſtimulus."

From this it would appear that the os tincæ, in conſequence of the irritation from the male penis, becomes the active ſource of venereal pleaſure. How does this tally with fact, and obſervation? We will inquire. The os tincæ, from the firmneſs of

its texture, and general want of ſenſibility, as far as can be determined by the touch, ſeems but ill calculated for this important office, more eſpecially as all women, as far as we know, have this part; but there are very many women who feel no pleaſure from this act, and others to whom the venereal congreſs is truly diſguſting, and yet theſe women are prolific. Now, unleſs it can be proven, that the os tincæ of the woman, who feels no pleaſure, and that of the one who does, be different, we muſt conclude, that the os tincæ is not the ſeat of venereal pleaſure. It perhaps may be doubted by thoſe who have not made the inquiry, whether this circumſtance obtains; but for the fact I can pledge myſelf, at leaſt, as far as the teſtimony of the women themſelves may go: inquiries were made of many, with the view of aſcertaining the fact; and they were conducted under ſuch circumſtances as could leave no room to doubt; the queſtions to this point were always anſwered with ſeeming candour, becauſe they were thought eſſential to the hiſtory of their complaints, or neceſſary to their cure. Beſides, women in the latter months of pregnancy, who do feel pleaſure from ſexual intercourſe, have equal enjoyment when the os tincæ is entirely obliterated, or out of the reach of the penis.*

We ſhould be glad to know, what is to be underſtood by the female feeling an unknown deſire; can we have a deſire

* This fact is of high import, and Dr. H. appears to have been aware of its force, ſince he endeavours to obviate it, by calling in queſtion, in ſome meaſure, the fact, by ſaying there is "a diminiſhed venereal ſenſibility," and a retraction of the cervix uteri from the vagina in the progreſs of geſtation; and hence it ſeems highly improbable that, during this period, the venereal orgaſm ever takes place.

If we underſtand what Dr. H. means by venereal orgaſm, we ſhall certainly inſiſt, that the gratification from ſexual intercourſe (in women who ordinarily enjoy it) is as perfect in the ninth month of geſtation, as in the firſt, or before it has even taken place.

and not know it? If this be not an Iriſhiſm, it is very much like one.

"Irritation applied by the glans penis to the os tincæ," ſays the doctor, page 423, "well prepared to receive this irritation by its projection into the vagina, and by its extreme ſenſibility, is the immediate cauſe of the venereal orgaſm, or that *unknown deſire* which the female ſometimes feels, in coition, for the ſeminal ſtimulus; and ſecondly, that an abſorption of the fecundating fluid, when applied to the os tincæ during the orgaſm, is the proximate effect thereof; and in the third place, that the tranſmiſſion of the ſemen to the ovaria, by the proper action of the uterine ſyſtem, is as naturally the conſequence of abſorption, as deglutination is of agreeable aliment taken into the pharynx. And laſtly, that impregnation is the final cauſe of the ſexual intercourſe, and of the pleaſure with which it is accompanied."

"It clearly follows from what has been ſtated above, that impregnation will never take place unleſs the venereal orgaſm has been excited; and it is equally plain, that it will not happen unleſs the ſemen is brought in contact with the os tincæ during the venereal orgaſm; and it follows with equal clearneſs, that an appoſition of the urethra to the os tincæ, at the ſame time will be important, if not indiſpenſable; not indeed for the purpoſe of the injection of the ſemen into the uterus, but that the ſeminal ſtimulus be applied to the os tincæ at the moment when it is prepared to abſorb it and convey it to the ovaria."

It has already been obſerved, that in our opinion the os tincæ has little or no ſenſibility; or certainly not that degree of it as would render it the ſeat of pleaſure, were it ever ſo well ſituated to receive the reiterated frictions of the penis; but it is by no means well ſituated for this purpoſe, as it is ſubject to a great variety of caſualties which will prevent its having a fixed place

within the pelvis. It therefore cannot from its "extreme ſenſibility" be the immediate cauſe of the venereal orgaſm, or "*that unknown deſire* which the female ſometimes feels, in coition, for the ſeminal ſtimulus;" firſt, becauſe it does not poſſeſs the ſenſibility which the author of "obſervations, &c." would attach to it; and ſecondly, that if even it did poſſeſs it, its uncertain ſituation within the vagina, would render it extremely difficult to profit by it. It is we conceive altogether an aſſumed principle to make the ſeat of venereal pleaſure reſident in the os tincæ; it is one that cannot be proven either by analogy, or experiment; for, on the contrary, this part has been touched an hundred and an hundred times by every practitioner of midwifry, without their having ever diſcovered it to poſſeſs "extreme ſenſibility."

But did it poſſeſs ſenſibility even to the extent the gentleman would wiſh, how is its form calculated to profit by it? In the unimpregnated ſtate of the uterus, but more eſpecially in virgins, this part is conical, with its apex looking outwards; its extreme part is drawn almoſt to a point; at its ſides ſpaces are offered by this part being pendulous in the vagina; the penis has with the os tincæ, (ſuppoſing it fixed,) five places of contact; above, below, on each ſide, and directly againſt its point; now ſhould "the velvet-like head of the penis" come immediately in contact with this extremity of the neck of the uterus, (which Dr. Harriſon inſiſts on being neceſſary that it may receive the ſeminal ſtimulus) what would be the conſequence; firſt, the penis would receive conſiderable injury from coming frequently againſt ſo firm a body as the os tincæ; and ſecondly, it would be productive of great pain to the woman, as happens when this contact does take place, in caſes of any degree almoſt, of precipitation of the uterus, by rudely forcing it upwards by the projection of the penis: pleaſure therefore cannot reſult from the extremity of the penis coming in contact with the extremity of the neck of the womb; if pleaſure then muſt be produc-

ed by theſe bodies touching each other, it muſt be by the penis paſſing from ſide to ſide, or from top to bottom of the neck, much after the manner a dexterous butcher whets his knife on a ſteel. But this mode of producing pleaſure, or the venereal orgaſm will not anſwer Dr. H.'s purpoſe, ſince the ſemen may be thrown on either ſide of the os tincæ, and of courſe would fail producing impregnation. "Had this fact and ſome others" we ſhall preſently ſtate, been known to Dr. H. he would not have adopted the hypotheſis which he labours ſo ſtrenuouſly to maintain.

By venereal orgaſm, we muſt underſtand a ſomething produced by, or the conſequence of pleaſure; and this effect is conceived abſolutely neceſſary to impregnation: we have already remarked, and ſhall now inſiſt that this is by no means neceſſary. Our reaſons for thinking ſo are, firſt, That many women are perfectly indifferent to the venereal congreſs; ſome affirm they never felt any thing like pleaſure, and others that it is not only diſguſting, but extremely painful: ſecondly, many women who labour under procidentia uteri have conceived, where the ſituation of this viſcus, (their own acknowledgements out off the queſtion) forbids the idea of pleaſure reſulting from ſexual intercourſe. But we think we have a right to inſiſt on the weight, which their teſtimony gives us, and this teſtimony goes directly to prove, that pleaſure is not neceſſary; nay, further, that conception has taken place in many inſtances of prolapſed uteri, where pain never failed to attend ſexual intercourſe. Thirdly, that, with other animals, ſuch as the dog, frog, newt, &c. on which the ingenious and accurate Spallanzani experimented, pleaſure or venereal contact was not neceſſary; his ſyringes were ſufficient, and they could hardly have been productive of pleaſure; the ſame we believe would obtain in woman. Fourthly, many inſtances have occurred where the ſecluſion of the vagina has been ſuch, as to prevent moſt effectually the "velvet-like head of the penis," and the exquiſitely

ſenſible os tincæ from coming in contact. Dr. H. has thought to provide againſt theſe obſtinate, and for him unfortunate facts, by ſaying, "that an appoſition may take place, appears to me entirely probable, (more probabilities! where are the reſponſes of the "coy maid" to theſe important interrogatories?) from the phenomena which very generally occur in a ſexual intercourſe, in which the venereal orgaſm is awakened; among which are the following: the cervix uteri becomes turgid (how do you know this?) the ligamenta rotunda contract;" (what proof have you of this?) have you not here aſſerted more than can be proven? Could this be proven, it unfortunately would very much militate againſt your hypotheſis, ſince the effect of this contraction would be to make the fundus uteri approximate the pubes, and conſequently make the os tincæ recede, or touch the ſacrum; this ſituation of the os tincæ would be very unfortunate for the production of venereal orgaſm, as it would be out of the way of the "velvet-like head of the penis."

"The uterus is depreſſed in the pelvis, and the os tincæ is brought nearer the orificium externum, and *aſſumes* a direction favourable to an appoſition;" how can it "*aſſume* a direction favourable to an appoſition," in caſes like thoſe recorded by Ruyſch, Mauriceau, Hilldanus, Harvey, &c.—where not only one, but more barriers oppoſed the introduction of the penis into the vagina? in theſe caſes we muſt regard the women who laboured under theſe diſadvantages as virgins in effect, and we can hardly ſuppoſe the uterus could have been ſo extremely accommodating as to crawl down into the pelvis and apply its os tincæ directly to the ſmall aperture that was free in theſe caſes, that it might open-mouthed receive the ejected ſemen; for here it muſt be remembered that no venereal orgaſm had taken place, ſince the "velvet-like head of the penis" was on the outſide of the grating, and could only peep through a very ſmall cranny at the os tincæ, anxious for "an unknown deſire," but which, moſt lamentable to tell, could not

be excited, becauſe there was a moſt obſtinate and envious partition between them. I would now candidly aſk this gentleman, if he can for a moment ſuppoſe, the uterus ſhall ſo far change its ſituation, in virgins more eſpecially, (for theſe women were phyſically ſo) as to have its os tincæ down to the os externum, or even within an inch of it, where no diſeaſe of the vagina, as far as we know, exiſted, to produce a procidentia uteri; I am ſure he muſt anſwer, he cannot. Theſe caſes will therefore furniſh moſt unequivocally the following concluſions: firſt, that it is not neceſſary to impregnation for the penis and os tincæ to come in contact. Secondly, that the venereal orgaſm, as far as is dependant on this circumſtance is not neceſſary to impregnation: and thirdly, that Dr. H.'s hypotheſis, which is entirely predicated on theſe circumſtances, muſt neceſſarily fall to the ground.

Having thus noticed Dr. H.'s theory of impregnation, we ſhall proceed to conſider his conjectures reſpecting ſuperfœtation.

After having expreſſed ſome doubts as to the poſſibility of ſuperfœtation taking place, Dr. H. obſerves: "But ſuppoſe ſuperfœtation may happen, we are then only prepared to offer ſome conjectures as to the manner in which it may happen; and we would aſk, why it may not take place as we have endeavoured to evince other impregnations do? This, it will be objected, will be impoſſible, the paſſage from the vagina into the uterus being obliterated after impregnation; but does it appear that this is the fact? Does not the ſmalleſt drop of blood effuſed into the uterus during geſtation eaſily find its way into the vagina? The fact is, the paſſage is only obſtructed with a viſcid mucus, which might, it is conceived, be diluted in time of coition, ſo as to admit the paſſage of the ſemen into the uterus, whence it may be conveyed to the ovaria by the fallopian tubes, as in other fecundations; for it will not be aſſerted,

that the openings of theſe tubes in the uterus are always obſtructed by the placenta."

I have choſen to give a complete quotation of Dr. H.'s theory, that no charge of miſrepreſentation might attach; by this opinion it is admitted, that a viſcid mucus does obſtruct the neck of the uterus, and that unleſs this be diluted, the ſemen could not have acceſs to the inſide of the uterus—how is the ſemen to get mixed with this mucus to dilute it? Does it poſſeſs any chymical power over it? If it does not, how will the thick tenacious fluid the ſemen, be made to act upon the viſcid mucus already preſent in the neck of the uterus, ſince, it will have to overcome the attraction of gravitation to remain an inſtant in contact with it? It may be ſaid, that the attraction of coheſion is ſuperior to that of gravitation; let it be admitted; the ſemen then can only act on that part of the mucus which is moſt inferior, and if it dilutes this, it will eſcape into the vagina, unleſs Dr. H. will point out ſome power in the uterus by which this inconvenience may be remedied. But we will not ſtick at trifles; we will allow the mucus to be diluted by the ſemen; but to effect this, it muſt be mixed with it, therefore the mucus and ſemen muſt travel together. What is there to obſtruct their journey? Firſt, theſe fluids muſt ſeparate the decidua from the uterus that they may paſs; how are they to do it? Secondly, when they have effected this ſeparation, they muſt continue it until they arrive at the fallopian tubes; when they get there they muſt clear the decidua from their mouths; how are they to do this? chemically, mechanically, or by a ſpecies of legerdemain? This Dr. H. is bound to anſwer and not we.

We have now ſuppoſed that theſe fluids have the power of ſeparating the decidua from the uterus, with which it is in very intimate contact; but will no injury reſult to the uterus from this ſeparation? Certainly, more or leſs hemorrhagy will enſue in the inſtant of ſeparation, and will flow into the vagina, for Dr. H. admits, that the "ſmalleſt quantity of blood effu-

ſed," will do this; there will then be counter-currents, the ſemen &c. paſſing up, and the effuſed blood downwards; where reſides the power to give theſe different directions? But it may be urged by the friends of this hypotheſis, although Dr. H. has forgotten to avail himſelf of it, that the decidua is porous and ſpongy, and that the ſemen, &c. will be tranſported by capillary attraction—this to be ſure is plauſible, and might, as a dernier reſource, be reſorted to, had not Dr. H. himſelf unfortunately deſtroyed all claim to the ſubterfuge, by admitting that the ſmalleſt drop of blood effuſed into the uterus during geſtation finds its way eaſily into the vagina. Where is the capillary power when this is allowed to take place? or will Dr. H. ſuppoſe, there is a peculiar and "unknown" kind of attraction, between the "mucus diluted by ſemen," and the decidua? Be this as it may, it appears to us not only as matter of great improbability, but of total impoſſibility, that the ſemen ſhould paſs in this way to the ovaries. There is then but one more way left for the ſemen to get to the ovaria through the uterus; and this ſuppoſes the animalcular diſcoveries of Leuwenhoek to be eſtabliſhed beyond doubt. We will ſuppoſe then, in conſequence of the union of the ſemen with the "viſcid mucus" with which the neck of the uterus is obſtructed, the latter becomes diluted; this is again ſuppoſed to be by ſingle elective attraction; that is, the mucus in which the animalculæ are ſuſpended, unites with the "viſcid mucus" of the os tincæ, and becomes diluted, and ſets them at liberty: they, as ſoon as freed from their bondage, by a ſpecies of inſtinct peculiar to themſelves, ſcamper through the meſhes of the decidua, travel through the fallopian tubes, and ſeek refuge and protection in the ovaria. I have thus offered a conjecture, at leaſt as plauſible as Dr. H.'s, to help him out of his difficulty; if he will not ſupport it, it is not my fault.

I am, Dear Sir, Yours,
With reſpect,
WILLIAM P. DEWEES.

Dr. John Redman Coxe.

Obſervations on the Cauſe and Cure of the Hemorrhagic State of Fever. *By* ELIJAH GRIFFITHS, M. D. *Read before the Medical Lycæum of Philadelphia. April 2d,* 1806.

MR. PRESIDENT, AND GENTLEMEN OF THE LYCÆUM,

THE ſubjects I have choſen for my eſſay, are ſome obſervations on the cauſe, and cure of the hemorrhagic ſtate of fever.

I believe, with the celebrated Ruſh, that the cauſe of hemorrhagy, is to be ſought for, in an increaſed action of the arteries; but that action is ſometimes ſo partial, as to ſcarcely ſhew any trace of general indiſpoſition. This is alſo the caſe in ſeveral other affections, that are called local; but which, unequivocally, reſult from increaſed arterial action; as in the commencement of phlegmonous inflammation, ſchirri, glandular ſwellings, ſome affections of the liver, &c.

The parts moſt liable to hemorrhagy, are, the nares, lungs, and uterus; perhaps the great vaſcularity, little lateral ſupport, and delicate covering to the veſſels of theſe parts, may be cauſes, why increaſed action of the veſſels here, is attended with rupture, inſtead of ordinary inflammation.

Blood loſt from the pulmonary veſſels, is very florid, coagulates ſoon, and from the great quantity, at times, diſcharged in a few minutes, I think it evidently arterial. Although the blood, in uterine hemorrhagy, comes from ruptured arteries, little difficulty will attend diſtinguiſhing it from the catamenia; which in healthy women is florid, preſerves its fluidity, and does not take on the putrefactive proceſs as ſoon as other

blood. It is moſt likely freer from lymph, and ſecreted by the ſame veſſels, which by a weaker action ſecrete the fluor albus; as this often precedes and follows the menſes.* The above obſervations, preclude the neceſſity of deſcribing diſeaſed evacuations from the uterus, and vagina: I may juſt add, that women of a delicate make, ſubject to dyſpepſia, conſtipation, and nervous diſorders, are very liable to uterine hemorrhagy.

We come now to make ſome obſervations, relative to that action of the blood-veſſels, on their contents, moſt favourable to maintaining the hemorrhagy when once eſtabliſhed.

As far as my own obſervations in this diſorder have gone, blood drawn from a vein early in the complaint, is very fluid, coagulates ſlowly, and conſequently exhibits a buffy ſurface. I hope preſently to ſhow, that this condition of the blood is cauſed by too much action in the arterial ſyſtem, and has been juſtly referred to an inflammatory diatheſis, ſo highly favourable to continuing hemorrhagy when once begun.

In Mr. Hewſon's experimental inquiries into the properties of the blood, many valuable obſervations are to be found on this ſubject: Vol. I. p. 61: he mentions that portions of the firſt and laſt drawn blood of a ſlaughtered animal, were received into ſeparate veſſels, when the laſt portions coagulated before the firſt.

The above experiment is by no means ſolitary, but has been repeated by the ſame gentleman, with a ſimilar reſult; and in-

* I have a patient now under my care, who has had a very copious flow of the fluor albus, for three or four months paſt, at the proper period of the menſes; but in no wiſe diſcoloured. She thinks it equal in quantity to the catamenia, and it preſerves its times of coming on, and going off, exactly as that evacuation did.

conteſtably proves, that the ſlow, or quick coagulation of the blood, is to be referred to the action of the heart and arteries, before it eſcaped from them; and that in animals almoſt bled to death, or near to ſyncope, the blood coagulates immediately after extravaſation.

If then the cauſe and continuance of hemorrhagy, is the effect of unequal and exceſſive action in the arterial ſyſtem, thereby giving more than uſual impetus, and highly attenuating the blood; the remedies are plainly ſuch as will diminiſh and equalize arterial action, viz.

1ſt. Blood-letting. This diminiſhes the quantity and impetus of the blood; it cauſes the action of contraction, in the blood-veſſels; and if carried to the proper extent, induces debility in the ſanguiferous ſyſtem, and a ſtrong diſpoſition to ſyncope; which favours the quick coagulation of the blood, and the formation of a clot in the mouth of the ruptured veſſel. Laſtly, veneſection by ſuddenly inducing general direct debility, and conſequent excitability, increaſes general, at the expenſe of local excitement.

We muſt however acknowledge, that there are ſome debilitated perſons labouring under this diſorder, in whom the functions of life are ſo proſtrated, as to forbid the uſe of the lancet; but this is no objection to the principle above ſtated, for we are acquainted with many other diſorders, accompanied by extreme general debility, where there is local increaſed action, till the laſt moments of life?

2d. The next claſs of remedies to be noticed are neutral ſalts. Among theſe ſaccharum ſaturni, and muriate of ſoda, have been the moſt popular. But how ſhall we explain the operation of medicines, when applied to one part, on remote parts of the body?

It will be proper in this place to make a few obſervations on the animal economy. I take it for granted, that the external ſkin, alimentary canal, and bronchial veſſels, are one extended ſurface; liable to take on aſſociated motions, from impreſſions made on any part of the nerves and blood-veſſels, ramifying theſe ſurfaces; theſe impreſſions may extend, not only to the ſanguiferous, and nervous ſyſtems, but to every part of the body, and have been termed ſympathy.

The doctrines of abſorption and ſympathy, both have their advocates. I ſhall leave this controverſy to the deciſion of future experiments, and proceed by a leſs intricate route, to the ultimate object of my eſſay. In the celebrated Mr. John Hunter's work on the blood, we find the following diſſection, which is quoted, becauſe I think it applicable, in the latter part of this eſſay.

Vol. I. p. 92—3. "Morgan, a houſe-painter, who had been paralytic in his hands and legs for a conſiderable time," came to his death by an accident. "On examining the body after death, the muſcles, particularly thoſe of the arms, had loſt their natural colour; but inſtead of being ligamentous and ſemi-tranſparent, as happens in common paralyſis, they were opaque, reſembling exactly, in appearance, parts ſteeped in a ſolution of Goulard's extract. From this caſe it appears that lead had been evidently carried along with the blood, even into the muſcles themſelves."

With a view to aſcertain what effects the exhibition of neutral ſalts would have on the blood, while circulating, I made the following experiments, in the preſence of ſeveral gentlemen, members of the ſociety, who aſſiſted me.

CASE I.

January 25, 1806, at 5 o'Clock, P. M.—J. L. a middle aged man was bled to ℥ iv. at the arm; ten minutes after he took muriate of ſoda ʒ ii. The blood ſhewed abundance of lymph and was coagulated very ſlightly at ſix o'clock P. M. when ℥ vi. more of blood were taken from the ſame orifice; the laſt portion began to coagulate in 5 minutes, and in eight minutes, was more complete than the firſt drawn portion, and without ſize.

CASE II.

January 25, 1806, at 4 o'Clock, P. M.—G. D. middle age, was bled to ℥ v: coagulation commenced in ten minutes with a ſeparation of much lymph. Fifteen minutes after being bled, he took ſaccharum ſaturni, gr xv. with ſugar and water. At half paſt 5 o'Clock, P. M. he loſt by the ſame orifice ℥ vi. of blood, which coagulated in 5 minutes, with very little ſeparation of lymph on the ſurface.

CASE III.

January 25, 1806, at a quarter paſt 4 o'Clock, P. M.—J. S. aged 35 years, loſt ℥ v. of blood from his arm, which began to coagulate in twenty minutes, with abundant ſeparation of lymph. Ten minutes after the operation, he took carbonate of ſoda ℈ ii. diſſolved in water. One hour after the firſt operation, he loſt from the ſame orifice, ℥ vi. more of blood. This began to coagulate as ſoon as it fell into the cup, and in leſs than 5 minutes formed a conſiſtent maſs, without any ſeparation of parts.

In every caſe we opened the vein as quick as poſſible, after applying the ligature, to avoid the effect on coagulation, of the blood's ſtagnating in the veſſels.

The above perſons were neither in good health, nor ſeriouſly indiſpoſed, at the time of bleeding them; but each of them had a tenſe, and rather full pulſe, at the firſt bleeding, which was ſomewhat reduced at the time of the ſecond.

On looking over Mr. Hewſon's work on the blood, ſince making the above experiments, I have ſeen ſome obſervations that have rendered it doubtful, whether the changes in the blood might not be owing to the bleeding, rather than the medicines. This determined me to teſt them, by the following experiments.

CASE IV.

February 23, 1806, 20 minutes before 4 o'clock, P. M.—B. W. aged 41 years, pulſe 76 and full, loſt ℥ vi. of blood from his right arm. The blood began to coagulate in 15 minutes and was very ſizy. Ten minutes after being bled, he took muriate of ſoda ʒ ii. Twenty minutes before 5 o'Clock, P. M. pulſe 76 in a minute and rather ſmaller; he loſt from the left arm ℥ vi. more of blood, which began to coagulate in 4 minutes without any ſizy appearance.

CASE V.

February 23, 1806, 15 minutes before 4 o'clock, P. M.—I. W. aged 45 years, pulſe 76 in a minute, was bled to ℥ iv. The blood began to coagulate in 15 minutes, but ſhowed very little ſize. At 5 o'clock, P. M. pulſe 70 ſtrokes in a minute: this perſon loſt ℥ v. of blood from his other arm, which began to coagulate in 12 minutes without any appearance of ſize.

CASE VI.

February 23, 1806, at 4 o'clock, P. M.—D. R. aged 49, pulſe beat 60 ſtrokes in a minute, loſt ℥ v. of blood from the left arm, which began to coagulate in 15 minutes, with very

little ſize. Ten minutes after the operation he took carbonate of ſoda ℥ ſs diſſolved in water. At 7 minutes paſt 5 P. M. pulſe 66 in a minute, he loſt from the right arm, ℥ v. of blood, which coagulated in ten minutes, without ſize.

CASE VII.

February 23, 1806, G. D. aged 34 years, pulſe 66 in a minute, at 10 minutes paſt 4 o'clock, P. M. was bled to ℥ vi. from his right arm. The blood began to coagulate in 15 minutes, but had little ſize on it.

At 13 minutes after 5 o'clock, P. M. pulſe 66 in a minute, he loſt from his left arm, ℥ vi. of blood, which began to coagulate in 13 minutes and was very ſizy.

CASE VIII.

March 9, 1806, 42 minutes after 3 o'clock, P. M.—D. R. aged 49, pulſe 75 ſtrokes in a minute, was bled to ℥ v from the right arm. The blood began to coagulate in 15 minutes, ſizy. Twenty-five minutes after 4 o'clock he took ſaccharum ſaturni ℈ i diſſolved in ſweetened water.

Seven minutes before 5 o'clock, P. M. ℥ v. more of blood were taken from the left arm, which began to coagulate in 17 minutes, more ſizy and firmly than the firſt portion.

CASE IX.

March 9, 1806, at 13 minutes paſt 4 o'clock, P. M.—G. D. aged 37 years; pulſe 78 ſtrokes in a minute, loſt ℥ v. of blood from the right arm; which began to coagulate in 10 minutes, ſizy.

Twenty minutes paſt 5, P. M. pulſe as before, loſt ℥ v. blood from the left arm; which began to coagulate in 15 minutes, more ſizy than the firſt portion.

CASE X.

March 9, 1806. P. C. aged 63 years, pulſe 80 in a minute and tenſe; complains of a catarrh. Was bled from his right arm to ℥ v. ten minutes before 4 o'clock, P. M. In twenty minutes he took carbonate of ſoda ℥ iſs. The blood began to coagulate in 35 minutes, and was very ſizy.

Fifteen minutes before 5 o'clock, P. M. ℥ v. of blood were taken from his left arm; pulſe 80 in a minute. The blood coagulated in fourteen minutes with very little ſize, and not ſo firm as the former portion.

CASE XI.

March 9, 1806. A. B. aged 46, pulſe 80 ſtrokes in a minute and a little tenſe; at 4 o'clock, P. M. loſt ℥ v. of blood from his right arm, which coagulated in twenty-five minutes, very ſizy.

At five o'clock, P. M. pulſe 80, he loſt from the left arm, ℥ v. of blood, which coagulated in 22 minutes, very ſizy.

Dr. Millar was preſent and aſſiſted me in the eight laſt experiments.

I think the foregoing experiments and obſervations will aſſiſt us in explaining the ſalutary operation of ſome of the neutral ſalts, on the heart and arteries, ſo as to favour the ceſſation of hemorrhagy.

If lead applied to the ſurface of the body, or lungs, as in the caſe of Morgan the houſe-painter, mentioned by Mr. Hunter, is capable of paralyſing a muſcle in the living body; we can hardly

heſitate to believe, that the ſame ſubſtance can exert a debilitating influence over the muſcular fibres of the heart and arteries, either direct or indirect.

Neither can we admit, that there exiſts any medium, through which lead can exert an influence over different parts of the body, that is not alike free for other active medicinal ſubſtances to exert their influence through.

That muriate and carbonate of ſoda, have a more decided operation on the heart and arteries, when taken into the ſtomach, than lead, is fully proven by the above experiments; and that the variety of appearances in blood, taken from the ſame individual, is to be referred to the mode of action in the blood-veſſels, before it eſcaped from them, is ſatisfactorily eſtabliſhed by Mr. Hewſon's experiments on the blood of ſlaughtered animals.

Many reſpectable gentlemen are of opinion, that the good effects from ſaline medicines in hemorrhagy, are to be aſcribed to their producing a nauſea at ſtomach, which is ſuppoſed to influence arterial action; but I hope ſoon to bring ſome circumſtances into view, that will render this opinion very queſtionable.

Mrs. F. about 30 years of age, the mother of ſeveral children, is of a very ſpare habit, ſubject to flatulency, depreſſion of ſpirits, &c. was taken with hemoptoe in October, 1805. The diſcharge was ſmall and ſoon over, but recurred at irregular periods, once or twice in 24 hours. She was free from cough, but complained a little of an unſettled pain, under and between the ſcapulæ. The pulſe was ſmall, weak, and frequent; tongue and ſkin natural. The menſes appeared at the uſual time, and the patient, with the above exceptions, was in the enjoyment of her ordinary health. Saccharum ſaturni was directed in ſmall

and repeated doſes, and continued for ſeveral days; but the complaint growing rather worſe, a large bliſter was applied to the breaſt, and the medicine increaſed to gr v. as often as the diſcharge of blood recurred. While the bliſter continued very ſore, and diſcharged freely, the hemorrhage was leſs, but immediately grew worſe on its drying up. Bliſters were now applied to the wriſts; theſe had a ſimilar effect as the firſt; but it was equally temporary, for on their healing the diſorder became greatly aggravated, and the patient would loſe in a very few minutes ℥ iii. or iv. of blood. The ſaccharum ſaturni was increaſed to gr viii. at a doſe; four or five of theſe doſes were taken in the 24 hours, and perſiſted in for two days, without preventing a return of the hemorrhage. Deſpairing of curing my patient with this medicine, carbonate of ſoda ℥ ſs. was diſſolved in half a pint of water, and a table-ſpoonful given every two hours; ſince that time there has not been the leaſt return of hemorrhagy. One ounce and a half of the medicine was taken in this way, when the ſoda and Peruvian bark were taken in combination, with much benefit to the patient. On the ſecond viſit after giving the ſoda, I found the pulſe ſlower and fuller; this was an increaſe of healthy general action, at the expenſe of local morbid action. Peruvian bark was adminiſtered at two different periods of the diſorder, with this intention, but had to be immediately relinquiſhed on account of its bad effects. No bad conſequences have reſulted from the lead, although between two and three drachms were uſed. The ſoda in this caſe produced not the leaſt nauſea, but on the contrary, removed the flatulency, and improved the appetite.

What I wiſh to offer in this place, is, that medicines given in doſes far ſhort of nauſea, produce their good effects, by an impreſſion made on the arterial and nervous ſyſtems; and it is only when they are given in larger quantities, that we witneſs a ſick ſtomach as a conſequence, and not a cauſe of the change in arterial action; it may be conſidered as a ſymptom, denot-

ing the ſtrong impreſſion our medicine has made on the blood-veſſels and nerves. I may further obſerve, that a univerſal languor, ſenſe of debility, and paleneſs, precede the operation of emetics; which makes it probable, that the medicine acts primarily on the arterial ſyſtem, through that infinite number of blood-veſſels, ſpread on the inner ſurface of the ſtomach, and then, through the nerves, the muſcular fibres of the ſtomach and diaphragm are thrown into convulſive action. If this be puſhed too far, it is no uncommon thing to ſee convulſions take place, in other parts of the nervous ſyſtem.

In ſupport of the above opinion, I may obſerve, that large bleeding, the application of tobacco to the ſurface of the body, certain paſſions and emotions of the mind, diſguſting ſights, &c. will all, at times, produce ſickneſs at ſtomach, and vomiting. None of theſe operate primarily on the ſtomach, but through the blood-veſſels or nerves; even vomiting produced by tickling the fauces, acts through the medium of ſenſibility, which is an attribute of the nerves.

From the above view of this ſubject, we can account for the ſalutary effect of a ſea-faring life, on a patient labouring under hemoptyſis, mentioned in Doctor Moſely's works; I mean the application of muriate of ſoda, to the ſurface of the body, and to the organs of reſpiration, for here was a ſurface for the medicine to act on, co-extenſive with Mr. Hunter's paralytic painter, whoſe muſcles exhibited ſuch ſtriking traces of the agent by which they were injured. It will not be contended in this caſe, that the ſaline particles were not applied, for I have found them depoſited on vegetables, at a very conſiderable diſtance from the ſea ſide, even when the weather was by no means tempeſtuous.

I have heard emetic medicines highly commended in this complaint, eſpecially in uterine hemorrhagy, but can ſay nothing on the ſubject from experience.

Cold has been uſed with great benefit in ſome urgent caſes of hemorrhage; in the form of cool air, cold water, vinegar, &c.

Reſt of body and mind ought to be obſerved, and if any exerciſe is permitted, it ſhould be very gentle at firſt.

Hemorrhagies ſometimes occur in the advanced ſtages of malignant fever. In theſe caſes there is a fault in the veſſels and their contents, the former become feeble, relaxed, and unfit to retain blood in its moſt healthy ſtate, much leſs, diſſolved incoagulable blood, which is often the ſtate of that fluid in malignant fever. Medicine can be of little uſe here, for if we can even arreſt the hemorrhagy, ſtill our patient is in a very hopeleſs condition; the only effective plan muſt be referred to prophylactics, or the ſucceſsful treatment of the firſt ſtages of the diſorder.

Obſervations on the Uſe of Arſenic. *By* JOHN REDMAN COXE, M D. *Read before the Medical Lycæum of Philadelphia, January* 30*th*, 1806.

GENTLEMEN,

THE following obſervations on the uſe of arſenic in the cure of diſeaſes, being chiefly of a practical nature, will not admit of that extenſive diſcuſſion ſo favourable to the intereſts of our ſociety; yet I hope they will prove of ſufficient importance to merit your attention to them.

The value of arſenic in the Materia Medica, does not appear to me to have been ſufficiently appreciated, although its ſafety has been long ſince eſtabliſhed by Dr. Fowler and other reſpectable practitioners. The terror of its name as a poiſon, is

perhaps the chief reaſon which can be aſſigned for its neglect; as I believe very few, if any, after giving it a fair trial, have ever rejected it from the catalogue of their moſt efficacious remedies. It is to be hoped, the following remarks will tend to remove any prejudices againſt its uſe; and that in future it will be more extenſively employed for the benefit of our patients, and of ourſelves.

The ſafety of arſenic will perhaps be beſt deduced, by a ſhort account of its exhibition in ſome few caſes, particularly in one, in which it was carried to an extent, both in time and quantity, not often known, and apparently with the beſt effects. This caſe, which I ſhall firſt detail, is that of a lady, who, for eight or ten years, had laboured under a leprous affection, which commenced about the fourteenth year, and was chiefly confined to the head, the body and upper parts of the arms. A variety of medicines had been exhibited both internally and externally, with but little effect. She applied for my advice in this complaint, in October 1802. She was alſo afflicted with a ſevere periodical headach about once in three weeks, to which ſhe had been ſubject for ſome years. At firſt I determined to try the efficacy of mercury with an intention of keeping her mouth ſore for a conſiderable time, and with this view I exhibited the mercurial pill, and employed externally a lotion of corroſive ſublimate. To this, I added the frequent uſe of the warm bath, and paid particular attention to the head, by having the hair conſtantly cut cloſe, and uſing daily a ſtrong lather of ſoap, together with friction with a piece of flannel. I continued this for nearly two months; the medicine exciting only a diſagreeable taſte in the mouth; but finding no advantage from it, I commenced the uſe of Fowler's ſolution of arſenic in doſes of 20 drops thrice a-day, and an ointment of corroſive ſublimate, and white oxide of mercury. The ſolution was increaſed to the amount of 50 drops, three times a-day in the ſpace of ſix weeks, at which doſes ſhe perſevered for ſeveral weeks, and at one time, for a pe-

riod of about three weeks, augmented it to 60 drops each doſe, which at laſt ſhe did not take the trouble to drop, but poured into a tea-ſpoon. This doſe, however, ſoon diſagreed with her, and ſhe found ſhe could not exceed the 50 drops even one drop in the doſe, without perceiving the unpleaſant effect of the medicine, viz. tumefaction of the face, nauſea and loſs of appetite, and a peculiar fulneſs of the eyes and giddineſs, which all indicated the propriety of diſcontinuing it for a time. The effect of the medicine on the complaint, was, however, rapid and ſurpriſing. In the courſe of a few weeks it had entirely diſappeared, and continued abſent for ſome time after the diſcontinuance of the remedy in conſequence of the ſymptoms above mentioned. The remedy had nevertheleſs been perſevered in the greateſt part of a year, with only the occaſional intermiſſion of a-day or two. At length the diſeaſe began to re-appear, but leſs extended, and the arſenic was again reſumed, in ſmall doſes, and with a ſimilar reſult as to its efficacy; and thus it continued diminiſhing under its uſe, and returning when omitted, during a period of nearly two and an half years. She has omitted the medicine entirely ſince laſt Auguſt, at which period, from her firſt employing it, ſhe had taken between a quart and three pints of it. My only rule in uſing it, was to continue it till the above-mentioned ſymptoms pointed out the propriety of omitting it. One curious circumſtance in the caſe is, that although at one time ſhe took 180 drops daily, or one grain and an half, yet, ultimately, ſhe could not bear more than five drops three times a-day, without in a few days feeling thoſe indicative ſymptoms of its noxious effects on the ſyſtem. This is, I believe, uncommon in the adminiſtration of any medicine; which we are obliged to augment in proportion as the ſyſtem becomes habituated to its uſe.

The check the complaint received in the firſt inſtance, and the immediate recurrence to the medicine on its re-appearance, ſeemed at length to induce ſuch a favourable change in the con-

ſtitution, that it entirely ceaſed on the body and arms, and was confined to the head, and that in a very ſmall degree. At preſent, after a lapſe of five months, the whole amount of the complaint in detached ſpots, does not equal the ſize of a dollar. I have purpoſely omitted the further uſe of the remedy till the ſyſtem may be fairly ſuppoſed to be entirely recovered from its influence; but I entertain the moſt ſanguine expectations, that eventually it will completely eradicate every diſpoſition to the recurrence of the diſeaſe.

One proof of its powerful influence, is, that the headach, which ſo ſeverely tormented her every three weeks, does not now occur more than once in ſix months, and that by no means ſo ſeverely as formerly: her whole appearance is more healthy, and her hair, which before grew thin and weak, is now greatly ſtrengthened and much improved, though this effect may alſo be in part owing to the frequent cutting, &c.

This caſe has completely convinced me of the ſafety of the medicine, both as to time and quantity. Its utility is well ſubſtantiated by Dr. Otto of this city, in a paper publiſhed in the Philadelphia Medical Muſeum, to which I muſt refer, as well worthy your attentive peruſal.

In the cure of intermittents, this invaluable medicine ſtands unrivalled. Even the bark in my opinion does not equal it: and indeed many caſes occur in which it is impoſſible to adminiſter the bark in ſufficient quantity, to produce the deſired effect, as in children; whilſt a few drops of the ſolution are adequate to the reſtoration of health in a few days. I have given it to ſeveral, of two, three, and four years of age, in ſome of whom the diſeaſe had continued three and four months, and was accompanied with enlarged livers, bloated appearance, &c. and always with the happieſt reſult.

To women with child, it alſo affords an invaluable and ſafe reſort in the cure of intermittents at all periods of pregnancy, whilſt the bark can rarely be adminiſtered, either from inability to retain it on the ſtomach, or from its being altogether improper at this intereſting period. I once adminiſtered it to a lady in her ſeventh month, in whom a moſt violent tertian threatened abortion. She was once bled, and after the ſecond paroxyſm commenced the uſe of the ſolution, in doſes of eight drops, three times a-day. The third fit was diminiſhed, and the fourth entirely ſuſpended, when elixir of vitriol and an infuſion of quaſſia completed her recovery. She went her full time, and was ſafely delivered of a remarkably fine child.

Theſe facts eſtabliſh the importance of this medicine, eſpecially in an intermittent country, where the high price of the bark, and the difficulty at times of procuring it, are inſuperable difficulties to its general uſe among the poorer claſſes of ſociety.

Of its utility in cancerous complaints I ſhall not ſpeak, although we have abundant reaſon to conſider it as the baſis of all the cancer powders, which at various times have been celebrated for its cure, although declared to be of vegetable origin.

I have lately been informed by a medical friend that he had found it very beneficial in two caſes of yaws; and when it is recollected that arſenic forms the baſis of the medicine ſo effectual for the cure of elephantiaſis, in the Eaſt Indies, we may reaſonably hope that it may prove equally efficacious in a diſeaſe probably a-kin to it.

I need not detain you with ſpeaking further of its uſe in many other diſeaſes, but ſhall merely ſtate my reaſons for preferring the ſolution to the arſenic in ſubſtance. Theſe are, firſt, that it is more uniform than any other preparation, and, ſecond-

ly, becauſe in this form it is to be viewed ſomewhat in the light of a neutral metallic ſalt, compoſed of the arſenious acid, and potaſs, (or an arſenite of potaſs,) which may reaſonably be ſuppoſed a milder medicine than the white arſenic, or, as it is in fact, the arſenious acid. I may remark here, the peculiarity attending the union of two bodies, in themſelves inert, (I mean oxygen and arſenic, for in its metallic ſtate it is ſo ſaid to be) in forming one of the moſt active medicines and poiſons, with which we are acquainted. So alſo is the union of quickſilver and muriatic acid, in the formation of corroſive ſublimate; as a medicine equally to be regarded, and as a poiſon, to be dreaded, with the compound we have been conſidering.

As to the modus operandi of this remedy, I ſhall not pretend to attempt an explanation. This, with many other points, it is not poſſible perhaps for our limited faculties to arrive at. That it is a tonic, its general effects in removing intermittents ſufficiently evince. That it is a ſtimulant, ſomewhat analogous to the tincture of cantharides, the following curious fact will perhaps render probable.

For ſeveral years I have been accuſtomed to employ the ſolution in the ſummer months, for the deſtruction of flies: 20 or 30 drops in about ℥ i. of water placed in a ſaucer, being adequate to this purpoſe. The aromatic ſmell of the lavender, probably entices them to taſte it; and in a few minutes it appears to excite an uncommon torpor in their ſyſtem, which allows them to be puſhed about, without any attempt to eſcape, and in a ſhort time they die without much apparent pain. Previous, however, to the torpor preceding the laſt moments of expiring life, an uncommon propenſity to the venereal orgaſm ſeems to be excited; for I have ſo repeatedly found them uniting in coition at this period, that I cannot doubt it being the effect of the arſenic; and in this actual ſtate of union they often die, as the torpor ſoon enſues which prevents their ſeparation, and which precedes death but a ſhort time.

I have thus, gentlemen, by a ſhort detail of facts endeavoured to draw your attention to the more general and extended employment of a medicine, which is a very favourite one with me, and which, I cannot but regard, as one of the moſt important in the Materia Medica; hoping that in your hands it may prove ſerviceable to thouſands; and confident, that it requires but a few trials, to make you as partial to it, as I am myſelf.

JOHN REDMAN COXE.

Philadelphia, January 30th, 1806.

On the Utility of Spirit of Turpentine in a ſevere Burn; and an Account of an extraordinary Inſtance of the late Commencement of the Catamenia. *By* DR. T. WATKINS.

Havre-de-Grace, 7th March, 1806.

DEAR SIR,

IF you think the following caſes of importance ſufficient to deſerve a place in your uſeful Muſeum, by inſerting them you will gratify,

Your friend and well-wiſher.

T. WATKINS.

DR. J. R. COXE.

SOME time in January, I was requeſted to viſit a child, who had ſuffered the moſt diſtreſſing accident from a burn, that I ever witneſſed. Being unfortunately from home, at the time the accident occurred, it was not until 24 hours afterwards, that I ſaw the child. He was between three and four years old, and playing too near the fire, his clothes caught it and

were entirely conſumed, before any perſon could come to his relief. His face, mouth, and ears were *parched* perfectly *criſp*; his arm-pits, arms and the fingers of one hand were in the ſame ſituation; and his breaſt, from the neck to the bottom of the ſternum, one entire ſore; his abdomen and lower extremities ſuffered but little injury, nor was his back at all burned. Nothing had been done but to give him a few drops of laudanum, and to cover the whole extent of the burn with dry cotton: upon the removal of this, I could perceive no inflammation, except on the breaſt where the burn was encloſed in a ſlight circle of redneſs. I ordered the turpentine liniment (recommended in your firſt Volume, and which I have always kept prepared) to be immediately applied to the whole injured ſurface; and as there was not the ſlighteſt appearance of fever, I conſented that his doſe of laudanum ſhould be repeated. As his fauces were conſiderably ſwelled, I ſuſpected that by the inhalation of the flames in crying, they had been burned internally, which upon inſpection I found to be the caſe; to relieve this, I directed that he ſhould hold in his mouth ſtrong ſpirits and water, as long as he could bear it, and repeat it three or four times a day. On the next day, as he had had no motion ſince the accident, he was ordered a gentle cathartic of ſulphur and cream of tartar, and the liniment was ordered to be repeated twice daily. Still no ſymptoms of fever and he appeared to be doing well. On the 3d and 4th days the dreſſings were repeated. He complained but little and appeared much inclined to ſleep. On the 5th day, as his parents deſpaired of his recovery, and indeed every moment expected his death, they omitted to apply the liniment, and when I ſaw him on the 6th, the whole wound had aſſumed a gangrenous appearance. I attributed this to the neglect of the preſcribed dreſſings, and requeſted that they would inſtantly apply bread and milk poultices, and renew them four or five times in the 24 hours. The effect of theſe is almoſt inconceivable, for when I ſaw him at the ſame hour on the next day, the wounds had recovered a healthy appear-

ance, and I again begged their ſtrict attention to the application of the liniment; this was continued for a few days longer, when I was compelled to leave home, and on my return a month afterwards, the child had perfectly recovered. I have had ſeveral opportunities of recommending this liniment in burns of leſs conſequence, with the moſt aſtoniſhing good effects.

During my viſit to a neighbouring county, I was requeſted to ſee a married lady, aged 42, who had been for ſome days complaining of headach, nauſea, pain about the loins, and all the ſymptoms of ſuppreſſed menſtruation. To a lady of her age I was aſtoniſhed that ſuch ſymptoms ſhould give any uneaſineſs, and expreſſed my ſurpriſe accordingly. She told me ſhe ſaw I laboured under a miſtake, which ſhe ſhould be compelled to rectify, however reluctantly; that ſhe had *never menſtruated*, nor had ſhe ever before felt the ſlighteſt ſymptom of any indiſpoſition attendant thereon; and that, therefore, ſhe was alarmed, being conſcious that the ſymptoms ſhe now laboured under, could not be the conſequence of *irregularities* in that way. You may judge of my ſurpriſe at her recital; I reflected that ſhe had arrived at that period of life, when many women *ceaſe* to menſtruate, and could not help entertaining a conjecture, that nature meant to renew her youth by bringing on her, thoſe inconveniences at the moment ſhe ought to have been freed from them. I bled her, requeſted her to take ſome mild cathartic, and took my leave. I viſited her again the next day, and to my no ſmall ſatisfaction as well as ſurpriſe, ſhe told me that the menſes were then on her, that ſhe felt relieved the moment they began to flow, and had continued ſo. The caſe was ſo extraordinary that I could form no comments on it and left her, apparently much pleaſed with her new viſitor.

Account of the ſingular Effects from the Bite of a Rattle-ſnake. By DR. S. T. BARSTOW, *Member of the Philadelphia Medical Society.*

Wilkeſbarre, May 27th, 1806.

DEAR SIR,

I HOPE you will pardon the liberty I have taken in thus addreſſing you; and if you deem the following hiſtory of a ſingular caſe, worth publiſhing, you may rely on its authenticity. It can be well atteſted.

Some time in the ſummer of 1801, the wife of Mr. Alfred Beeman, in the town of Braintrim, Luzerne county, Pennſylvania, was bitten by a rattle-ſnake: ſhe was then in the fourth or fifth month of pregnancy. After ſome conſiderable degree of the common conſequences of ſuch an accident had occurred, ſhe at length recovered; at the full time of delivery ſhe was ſafely put to bed. The child was apparently healthy; but immediately after allowing it to ſuck, it aſſumed the hues of a rattle-ſnake, ſwelled very much, and ſoon died. She then procured a puppy, which died in two days of the ſame ſymptoms. A lamb was tried next, and in ſucceſſion, one puppy and three lambs ſhared the ſame fate. Another puppy was procured, which diſcovered but little of the ſymptoms, and did not die. The woman all this time was as well as uſual, under a convaleſcence from child-birth. In the ſpring of 1803, I was called to ſee Mrs. Beeman, ſoon after another birth. She had ſtrong apprehenſions that it would be unſafe to put her child to the breaſt, but was prevailed on to make the experiment, and no diſagreeable conſequences reſulted. I had loſt a part of the circumſtances of this ſtrange fact, ſo that I could not communicate it during my reſidence in your city, the laſt winter. I have now obtained a repetition of them, not only from Mr. Beeman and

his wife, but from ſeveral perſons of veracity beſides. Among theſe is H. V. Champins, Eſq. on whoſe teſtimony I can fully rely, and who has authorized me to uſe his name in this place.

I am, Sir, very reſpectfully,

Your obedient and humble ſervant,

S. T. BARSTOW.

DR. J. R. COXE.

A ſingular Caſe of Abſceſs. *By* DR. CHARLES SMITH.

New Brunſwick, (N. J.) March 7, 1806.

I WAS called on a few days ſince to viſit the infant child of Mr. W. Thrackmorton, of Spottſwood, which was reported to have an abſceſs formed on its back, which required opening. A few minutes before my arrival, Mrs. Thrackmorton, on examining the tumour, perceived ſomething like a ſharp point diſtending the ſkin from within, which induced her to touch the part with a needle, on which nearly a tea-cupful of pus ruſhed out; on wiping away the matter, a ſharp point was ſeen to protrude from the orifice of the abſceſs, which was taken hold of and withdrawn, and proved to be a head of rye without the grain, with nearly all the ſpiculæ or beards entire, and about half an inch of the ſtem or ſtraw: this was performed in the preſence of Mrs. Perrine and Miſs Gordon the nurſe. I meaſured this head of rye, while wet with matter, and found it to be rather more than four inches and three quarters long. The

abſceſs was ſituated about an inch and a half, or two inches on the right ſide of the ſpine in the lumbar region. The above caſe appears as ſingular as any which has hitherto fallen under my obſervation. Is it poſſible that this extraneous body could have been ſwallowed, or otherwiſe have entered the body of this child unleſs by deſign? I judge not. Indeed I do not think it could have been introduced into the ſtomach in any way, without extreme diſtreſs, if not immediate ſuffocation. The only ſolution of the phenomenon which preſents itſelf to me is, that it muſt have been introduced into the rectum, with a deſign to deſtroy the child. A female ſervant of the family, of a malignant diſpoſition, is ſuſpected of having committed this crime four weeks before my viſit; this ſervant had the care of the child for a few hours only: from this time it became reſtleſs and uneaſy, and continued to diſcover ſymptoms of great diſtreſs, until the offending cauſe was removed. I examined the abſceſs, and diſcovered the ſinus to be very deep and evidently leading into the cavity of the abdomen.

CHARLES SMITH.

DR. J. R. COXE.

Caſe of Epilepſy relieved by Acetate of Lead;—and of a diſeaſed Eye.
By DR. H. JAMESON.

Gettysburgh, March 6th, 1806.

DEAR SIR,

I SUBMIT to your better judgment, whether the two following caſes of diſeaſe are worth a place in your Muſeum: leaving you perfectly at liberty to publiſh, or ſuppreſs them; I proceed to relate,

A Caſe of Epilepſy, which was conſiderably checked by the Uſe of Acetate of Lead.

Peter Moon, of Lancaſter county, applied to me for a caſe of epilepſy, in the fall of 1802; he remained in my care till Auguſt, 1803: during this time, he underwent a ſevere ſalivation from mercury; took the cupr. ammoniacum, &c. &c. all to no purpoſe. Having been attacked with the complaint after manhood, I ſtill entertained hopes that ſomething might be done, to relieve him from ſo wretched a diſeaſe; I therefore ſent him to Doctor Ruſh, for his opinion; who kindly condeſcended to inform me, that he had cured epilepſy with the ſac. ſaturn. and recommended it in this caſe; I accordingly began with it in ſmall doſes, and increaſed it gradually to eight grains per day, without inconvenience to my patient. The patient expected to have his fate preſaged by Dr. Ruſh, and on finding that I promiſed him no certainties, but ſtated fairly to him, what the doctor had written me, he was not much delighted with the proſpect; however, he put himſelf under my care, and took the medicines as above ſtated, for three weeks, during which time he had a moſt violent fit, which altogether diſcouraged him. A blackſmith about this time, was famed for having effected, as was ſuppoſed, ſome cures; and holding out a certain cure to this young man, he deſerted me and became the patient of the ſmith, who, with great formality, put an iron ring on his wriſt, to the unſpeakable happineſs of the patient: he had no fit afterwards for four months, although he formerly had them every two, three, or four weeks. I left Lancaſter county ſhortly afterwards, and therefore know not how the caſe ended. The above caſe needs little comment; he took no medicine but the ſugar of lead; he went right away from me to the ſmith; there was an interval of four months; and as to the return, what was more likely to act as an exciting cauſe of his diſeaſe, than exceſſive labour? He had undertaken to clear a piece of land by the job, and worked very had for many days before the fits return-

ed; and luckily for the ſmith and ſuperſtition, the patient had taken off the ring that very day, before the fits returned, in conſequence of its being inconvenient, and ſuppoſing himſelf cured. To me there is no kind of doubt, but the ſac. ſaturn. poſtponed the diſeaſe the above length of time, and that it would have effected a cure.

Caſe of diſeaſed human Eye, in which a hard Subſtance was got.

I WAS called to ſee Mrs. Ahey, of Lancaſter county, in 1803. She was labouring under a hectic fever and excruciating pain in her left eye, which was grown to nearly four times its natural ſize; the eyelids were ſtretched to the thinneſs of paper; the cornea was wholly obliterated; the whole eye had loſt its natural colour, being now of a blackiſh or dark brown colour; the tumour was yielding to the touch: I therefore thought an inciſion into the tumour would leſſen its ſize, by letting out a conſiderable quantity of a fluid, which I thought muſt be preſent from the feel of the part. Very little was diſcharged; but in making the inciſion, I cut upon a hard ſubſtance, which induced me to extirpate a conſiderable part of the eye or tumour. The operation was attended with great pain, but afforded great relief from pain afterwards; but the fever with which ſhe was affected, grew gradually worſe, and in about three months carried her off. On examining the part of the eye, which I had taken off, I found the whole cavity filled with a ſubſtance reſembling tar, in colour and conſiſtence, and within the poſterior chamber of the eye, I got a hard ſubſtance which I had ſince preſerved, and intended to have incloſed, but have unfortunately miſlaid it while writing this.* The patient informed me, that ſix years previous to my viſit, ſhe was

* Should the doctor be ſo fortunate as to find this ſubſtance, he will confer a great favour in tranſmitting it to the Editor.

ſtruck with a corn-ſtalk in loading fodder, that produced great pain and violent inflammation, but to her knowledge there was no opening into the eye; that the inflammation had yielded to common remedies which had been preſcribed by a phyſician in the neighbourhood; that crabs eyes were put into the eye to clear it of dirt. On examining the ſubſtance, I was decidedly of opinion, that it was a cruſtaceous animal which had grown in the eye; it appeared to be compoſed of irregularly triangular, hollow pieces of ſhells, joined together with a pretty firm ligament at each joint, which were cloſe together, large at one end and ſmaller at the other. It would probably have weighed about 15 grains, and moved freely in the way of a common hinge. Having been broken in the eye, I cannot be very poſitive in my opinion, of its being a peculiar piece of organization, or ſome part of a crab; but ſure I am, it was not a crab's eye: poſſibly a claw; if ſo, it was lodged ſix years within the eye. What muſt the patient have ſuffered? What do not mankind owe to ingenious gentlemen who perform ſuch cures as the worthy Dr. Phyſick has in the caſe of your little ſon? And what pain might this woman have eſcaped in the hands of that great man, on timely application?

With ſentiments of eſteem,

Your's, &c.

HORATIO G. JAMESON.

DR. JOHN REDMAN COXE.

Remarks on DR. WENDELL'S *Caſe of Worms; and on the Efficacy of Nitrate of Silver in Epilepſy. By* JOHN PARKER GOUGH, M. D.

CHARLESTON, (S. C.) *May* 31*ſt*, 1806.

DEAR SIR,

I HEREWITH tranſmit for your Muſeum, ſome *curſory remarks* upon Dr. Wendell's caſe of worms in the *bladder*, which appeared in your laſt number, and alſo an account of the efficacy of the nitrate of ſilver in a caſe of epilepſy. If you think theſe articles deſerving a place in your uſeful work, they are at your ſervice. Wiſhing you, ſir, a continuance of that ſucceſs which has crowned your exertions, I beg leave to ſubſcribe myſelf,

Your's, with eſteem,

JOHN PARKER GOUGH.

JOHN REDMAN COXE, M. D.

THE caſe related by Dr. Wendell, of *worms* being found in the *bladder*, is ſo ſingular, that it cannot but excite the attention of all who peruſe your valuable and intereſting publication. I confeſs that I was not a little ſurpriſed at ſo *uncommon* an occurrence; but the *more* I have reflected upon it, the *more* I am ſatisfied, that the Doctor is *miſtaken in ſuppoſing thoſe ſubſtances* which paſſed from the bladder *to be worms*.

As truth ought always to be the object of our reſearches, and it being only by a colliſion of ſentiment, that we can arrive at any principles in ſcience, I ſhall be excuſed for offering an opinion upon a *ſubject* that ſeems to be of ſome *practical* importance.

I ſhall not here notice the ingenuity of Dr. Wendell, in explaining the *manner* theſe ſubſtances got admiſſion into the bladder; becauſe, *it is difficult to conceive*, and indeed reaſon *rejects* the ſuppoſition, *that a living body could perforate the colon, and ſo ſenſible an organ as the kidney, without inducing conſequences fatal to the patient, and that perhaps immediately.* Neither is it to be ſuppoſed, *even* admitting his explanation, *that a worm could ſo far change its nature and aſſimilate itſelf to its new element, as to exiſt for any length of time, in a ſaline, acrimonious fluid, like the urine.*

That theſe ſubſtances were not worms, I infer, firſt, from the conſideration of their not being *poſſeſſed of life.* Dr. Wendell does not ſay that they were *alive* at the time of their extraction, and there is *no reaſon* to ſuppoſe they were ſo *in the bladder*, as the means by which they were drawn forth, *could not*, it is apprehended, have deprived them of exiſtence. The circumſtance of the patients being ſenſible of a "vermicular motion" in his bladder, is no objection to this opinion, becauſe the contractile power of this organ, naturally great, acting on *elaſtic bodies*, as theſe ſubſtances are acknowledged to be, and ſtimulated by them into uniform and inordinate action, is ſufficient of itſelf, it appears to me, to account for this extraordinary ſymptom.

Secondly. I am induced to believe that theſe ſubſtances were not worms, becauſe, in *ſome* caſes of *hematuria*, bodies have been occaſionally voided from the bladder, bearing ſo ſtrong a *ſimilitude* to worms, as to be taken for them.

In this caſe we well know the deception ariſes from blood being depoſited in the *ureters*, and by coagulation, acquiring the form of thoſe veſſels, and thereby creating the reſemblance in queſtion. This fact did not eſcape Dr. Cullen. In his "firſt lines" under the preſent article, he ſays "it ſometimes alſo hap-

pens, that the blood *ſtagnating* and *coagulating* in the *ureters*, takes *the form of theſe veſſels*, and is therefore voided under the *appearance of a worm*; and if the coagulated blood happens to have, as it may ſometimes have, *the gluten ſeparated from the red globules*, theſe *worm-like appearances* have their external ſurface *whitiſh*, and the whole ſeemingly forming a tube containing a red liquor."

Here then, we have a ſolution of this caſe, at leaſt, this is the explanation I would offer; whether correct or not, I leave to others to determine. Mr. Delamatter had *probably* been the ſubject of *hematuria* at ſome time antecedent to this attack, or he might have been *occaſionally* afflicted with *gravel*, which, in its paſſage through the *ureters*, may have *eroded* their ſurface, and the blood *effuſed in conſequence*, laid the *foundation* of theſe worm-like appearances. The ſame effect may have been alſo produced by *ſome accident or violent exertion occaſioning the rupture of a ſmall blood-veſſel in theſe parts*.

I am the more confirmed in the opinion *I have* endeavoured to ſupport from this additional circumſtance, that the *ſhape** of theſe *ſuppoſed* worms correſponds with that of the *ureters;* the diameters of which, if my memory ſerves me, are larger as they proceed from the *kidney* than near their inſertion in the bladder. The exquiſite pain, &c. which the patient felt in the *lumbar region*, was doubtleſs owing to *the paſſage* of theſe ſubſtances *from* the ureters *into* the bladder.

And indeed the ſymptoms in general deſcribed by the doctor, are by no means peculiar. They are the uſual concomitants of affections of the *urinary ſyſtem*, and *no more* happens here, than *commonly* takes place from the introduction of *any* extraneous body into the kidney or bladder, as in "Nephralgia Calculoſa."

* Dr. Wendell in his deſcription ſays, that one extremity of theſe ſubſtances was larger than the other.

P. S. Since writing the above, I recollect a caſe related by Dr. Barry in the ſixth volume of the Edinburgh Medical Eſſays, of a worm being paſſed from the bladder of a man labouring under *hematuria*. But the hiſtory of this caſe was *materially* different from the one before us. The worm was ſmall, and upon examination was found to have both mouth and eyes; and circumſtances too, rendered it highly probable, that it had formed a habitation for itſelf, *not in the bladder*, but in the *extremity* of a blood-veſſel; and that it derived its nouriſhment from *the blood* and the *blood only*. It is alſo probable that as ſoon as it found a paſſage into the bladder it ſuffered an extinction of life, for it was voided dead; the urine not being a fluid adapted to its nature.

Caſe of Epilepſy, cured by the Uſe of Argentum Nitratum.

Mary-Ann, a ſervant girl belonging to T—— L——'s, Eſq. between 17 and 18 years of age, apparently of a good habit of body, was placed under my care for this diſeaſe in the ſummer of 1803. Upon inquiring into the hiſtory of her caſe, I found that it had commenced with her periodical diſcharge about two years before; at which time ſhe was ſeized ſuddenly without any premonitory ſymptom, or evident exciting cauſe, and had been afflicted with it more or leſs ever ſince. During this term, ſhe *regularly* menſtruated, though not always ſufficient in quantity. It was obſerved, however, that no abatement of the violence of the fits was in conſequence perceptible; nor did this ſtate of her body ſeem to have any effect in lengthening the intervals between them.

This diſeaſe did not appear to obſerve any regularity in its attacks; for ſometimes ſhe would have it every four or five days, ſeveral times in ſucceſſion, and again ſhe would be free of

it for one or two weeks together. In other respects she enjoyed a good share of health with the exception of a pain in her head, which distressed her occasionally, leaving a sensation of fulness afterwards.

In order to remove the effects of a plethora under which she seemed to labour, and which is generally a predisponent cause of epilepsy, I bled her freely, and gave an emetic to cleanse the "primæ viæ." Her body was kept open by cathartics, and the regimen usual in such cases was pursued.

She was put upon the use of metallic tonics combined with bark, and ordered moderate exercise and a change of air. I also inserted a seton in her neck, which was kept running. Finding that this plan of treatment did not equal my expectations, I determined to make trial of the "argentum nitratum," which I knew had been favourably spoken of, and accordingly began with half a grain twice a day, in the form of pills, increasing the dose by degrees. The success of this experiment answered my most sanguine wishes. A fit, which she had a few days after this remedy had been adopted, was evidently shorter in its duration, and less violent in its effects. I now looked forward to a most happy result, nor was I disappointed in my expectations. She began to mend very fast; her fits were less frequent; the pain in her head, of which she complained so much, left her, and in a few weeks she was completely restored to health. The pills produced no other sensible effect than that of rendering her costive, which was obviated by the use of occasional doses of medicine.

She has never since had a return of her disease.

Extract from the Report of the Board of Health of Philadelphia, to GOVERNOR M'KEAN, *on the late Epidemic of that City: with Remarks on the ſame, by a Phyſician to the Southward. In a Letter to the Editor, dated January* 28, 1806.

"FROM the very great precautions which were taken at the Lazaretto, to examine and purify all veſſels from ſickly ports, or that had ſickneſs on board; and the very particular care which was taken to remove from the city and its vicinity, all ſuch ſubſtances as might have a tendency to contaminate the air; the board flattered themſelves that the city and liberties would have eſcaped the calamities inſeparable from the prevalence of a malignant fever this year, as they had the laſt. Theſe expectations however, were not realiſed, for, on the 30th of July, they received information that two perſons were ill of a fever, with malignant ſymptoms, at the houſe of Samuel Criſman, who kept a retail grocery ſtore at the north-eaſt corner of Catharine and Water-ſtreets, in Southwark, and that one of Criſman's apprentices was ill at his parent's, in the Northern Liberties. The two perſons that were ill at Samuel Criſman's were immediately ſent to the Lazaretto, where one of them died with highly malignant ſymptoms on the 3d of Auguſt; the reſt of Criſman's family retired to the country the day after the removal of the ſick perſons, and his houſe was cleanſed and ventilated.

"At the time the ſick perſons were ſent from Samuel Criſman's to the Lazaretto, there was no other caſe of malignant fever in that neighbourhood, or in any other part of Southwark, and by the confeſſion of Peter Young, one of the ſick perſons, on his death-bed, in the preſence of ſeveral witneſſes, they had made

a clandeſtine viſit to the Lazaretto the Sunday before they were attacked with the diſeaſe.

"At that time ſeveral veſſels from different ports of the Weſt Indies were performing quarantine at the Lazaretto, on board of ſome of which, perſons had been ſick, and had died of the yellow fever, and among others, the ſchooner Nancy, captain Lake, from the city of St. Domingo, with a number of wounded French ſoldiers, who had been received on board from a military hoſpital, ſoon after which the whole of the crew, excepting two, became ill of the yellow fever; one died at ſea after three days ſickneſs, and the captain and one ſeaman were landed dangerouſly ill at the Lazaretto, according to the bill of inquiry and an extract from the log-book of the veſſel tranſmitted to the health office.

"For ſix or ſeven days after the attack of the perſons at Criſman's, no other caſe of malignant fever, within the knowledge of the board, occurred in that neighbourhood, or in any other part of Southwark, the City, or Northern Liberties, and after that period, it made its next appearance in the three neareſt families to Criſman's; it alſo re-appeared on the 8th of Auguſt in Criſman's family, which had returned to the ſame houſe, contrary to the intentions of the board, on the fifth; and a few days later in two or three families on the oppoſite ſide of the ſtreet, and to two in a neighbourhood more remote; and, was evidently propagated from one to another, and afterwards conveyed to other neighbourhoods, in the ſame manner as diſeaſes univerſally acknowledged to be contagious, are uſually propagated, and not like a diſeaſe derived from noxious effluvia diffuſed in the atmoſphere, to numbers in different directions at the ſame time, nor like a diſeaſe occaſioned by foul air proceeding from a particular ſource, which neceſſarily ceaſes to ſpread, or even to exiſt, ſoon after the original ſource from which the foul air proceeded, has been removed or corrected.

"As ſoon as the board received intelligence of the exiſtence of the diſeaſe, they uſed every exertion, ſhort of abſolute compulſion, to remove the ſick and the families with whom they reſided, to houſes provided for their reception in the country. They likewiſe adviſed and urged the immediate removal of all the families in, and near, the neighbourhood where the diſeaſe exiſted; and endeavoured to arreſt the progreſs of the diſeaſe, by preventing, as much as in their power, unneceſſary intercourſe with the ſick, and by warning people, reſiding in other parts, from viſiting or tranſacting any buſineſs in the infected ſtreets.

"Meaſures ſo well calculated to prevent the diſeaſe from ſpreading, could not have failed of ſucceſs, as was proved by the proceedings in Providence, Rhode Iſland, if the people had not unfortunately been impreſſed with an opinion that the diſeaſe was not contagious, and of courſe that no precautions were neceſſary. Miſled by this opinion they diſregarded the regulations and admonitions of the board, in conſequence of which the diſeaſe was gradually conveyed into other families, and into ſtreets more remote, and though the increaſing mortality ſoon convinced the people that the diſeaſe was of a different and more dangerous nature than they had been taught to believe, and occaſioned the precipitate flight of multitudes, it had become too widely extended before this conviction took place, for the ſubſequent regulations and exertions of the board to eradicate it, or entirely ſuſpend its progreſs; they, however, have the ſatisfaction of believing, that notwithſtanding the oppoſition and difficulties with which they had to contend, they were inſtrumental, under DIVINE PROVIDENCE, in retarding its progreſs, and in reſcuing and preſerving a number of valuable lives in Southwark, as well as in preventing the diſeaſe from pervading the City and Northern Liberties, and involving the inhabitants in the complicated miſeries of former years."

REMARKS ON THE FOREGOING.

EVERY publication on the ſubject of an awful epidemic, which has ſo often ravaged the towns on the Atlantic ſhore, and weſtern waters, will command the attention of all the anxious friends of their country: the official ſtatements of the public health officers, are in an eſpecial manner entitled to particular conſideration, becauſe it is to be ſuppoſed, that, having the beſt opportunities of obtaining correct information, they will give all the facts neceſſary to the developement of the origin of the ſcourge, ſubſtantiated by every legal teſtimony, and uninfluenced by private theories or opinions.

Philadelphia has ſuffered much more than the other cities and towns of the United States, and ſcarce a year has paſſed ſince 1793, without alterations of the health law of the ſtate, which no doubt were made in conſequence of the experience of the ſuppoſed defects of the former acts. The medical gentlemen of Philadelphia have alſo repeatedly addreſſed the governor on the ſubject, and furniſhed all the hints in their power to give, in order to preſerve the public health from diſeaſes, as well of foreign as of local origin. Hence the attention to cleanlineſs of the city has been increaſed to a degree, which raiſes the admiration of every ſtranger; while the quarantine on the ſhipping has been gradually made ſo ſevere as to drive away a great part of its former commerce, (eſpecially the very profitable coaſting trade), while the rival cities of Baltimore and New York, by more judicious quarantine regulations have trebled both their foreign and domeſtic buſineſs, and have been partially affected by diſeaſe. The legiſlatures of thoſe ſtates appear to think that there is a point to which human prudence ought to extend, and that to attempt any thing more would be as abſurd as ruinous. But to the point.

To prove the importation of the diſeaſe, appears to be an object of conſequence with the board; and in order to do this, they bring forward the teſtimony of Peter Young, to trace the contagion to the viſit of himſelf and his companion to the Lazaretto, 15 or 16 miles ſouth of the city, where people from the Weſt Indies were then ſick with the yellow fever. But why, it may juſtly be aſked, is the teſtimony of Peter Young, *who is dead*, referred to, while his fellow apprentice, who accompanied him to the Lazaretto, is alive, and of courſe able to afford all the requiſite information, and to ſubſtantiate the aſſertions of Young? Does not the reference to *dead teſtimony*, and the neglect to bring forward that of living perſons, ſavour of a wiſh to preclude the poſſibility of obtaining a correct ſtatement? It is alſo to be hoped, that the board have taken pains to receive the depoſitions of thoſe witneſſes before whom it is ſaid Young made the confeſſion of his viſit. Beſides, the board omit to ſtate in what mode the boys were conveyed to the Lazaretto. Did they ride in a carriage, or did they row down in a boat? Did they return before night? Was the day extremely warm? What was their daily employment before and after the viſit? The board may ſay, why inquire into circumſtances which we do not believe could in the leaſt influence the queſtion of origin? It may be anſwered, becauſe the private belief of the board will not, and OUGHT NOT TO HAVE ANY WEIGHT WITH THE PUBLIC; and the chief object which renders the annual addreſs to the governor of any importance, are the FACTS, which it is to be ſuppoſed they have in their power to collect, and upon which an opinion may be formed, as to the ſource of the complaint. To the phyſicians in the ſouthern ſtates, who may be anxious to know the truth, it is all important to aſcertain the above facts; becauſe, being accuſtomed to witneſs the operation of the combined influence of the violent heat of a July ſun, marſhy exhalations, of great fatigue, with or without ſubſequent expoſure to night air, in the production of malignant fevers, they will have no difficulty in accounting for the attack of fever in the perſons of Young and

his companions, without having recourſe to the bug-bear contagion. They have philoſophy enough to know that the ſame cauſes will produce the ſame effects, whether in operation in Pennſylvania, on Savannah river, on the Ganges, or on the Miſſiſſippi. At any rate, the board will not ſurely deny the importance of aſcertaining, whether the boys who viſited the Lazaretto, actually went into the room of the people ſaid to be ſick with the yellow fever; becauſe if they merely went on ſhore to view the Lazaretto, without being expoſed to the contagion, and if the power of the above cauſes to produce fever be denied, where, in the name of common ſenſe, was the harm of the viſit? I contend that the expreſſion of the board is an *equivoque*, and requires an explanation, before any weight will attach to it.

The circumſtances of the appearance of the diſeaſe in the "three neareſt families to Criſman's," though carefully ſtated in order to favour the opinion of the epidemic having originated from his houſe, yet fails of effect; nay, ſome facts ſtated by the board themſelves, and others which they have omitted, contradict the idea. It is ſtated that the *ſick were immediately removed (and doubtleſs their bedding with them,) and the houſe cleanſed and ventilated:* how then could the diſeaſe be propagated by contagion to the neighbourhood? Did the members of "the *three neareſt families*" viſit the ſick during their temporary continuance in Criſman's houſe? If not, how could they take the diſeaſe from Criſman, after his return from the pure air of the country to his houſe, which was ready cleanſed for him? Did he infect the family to which he retired in the country with his clothes charged with the ſuppoſed contagion, derived from his ſick boys? Surely ſuch an event ought to have been expected, if it be believed, as the board believe, that the contagion may be carried from the Weſt Indies to the Lazaretto, and from the Lazaretto to the city. And why did not the other ſick boy, who retired to his parents in the Northern Liberties, alſo ſpread the ſame diſeaſe there? The ſilence of the board, as to the firſt

point, fully proves that the friends of Criſman in the country remained well; and though 18 caſes are ſtated to have appeared in the Liberties, yet it is not pretended that any of them were derived from the apprentice of Criſman. As the fact of the introduction of the diſeaſe into the Northern Liberties by him, would have been an *argumentum crucis* in favour of their doctrine of imported contagion, there can be no queſtion but that the moſt minute inquiries were made to aſcertain it, and their ſilence on the point, proves inconteſtably that their inquiries were unſucceſsful. Whence then did the 18 ſick perſons in the Northern Liberties acquire their diſeaſes? The board, who trace the importation of the fever ſo logically into Southwark, are bound to ſatisfy unintereſted inveſtigators on that queſtion. The board ſhould account for this partial operation of contagion, which can remain ſo active all a voyage, take effect in the ſouthern extremity of the city, and not in the northern, and die when carried a few miles out of town! Is THIS LIKE THE SMALL POX? Is IT LIKE ANY CONTAGIOUS DISEASE IN THE WORLD?

Conſiſtently with the preconceived opinions of the board, as to the origin of the epidemic, they proceed to ſtate the progreſs of it in the neighbourhood of Criſman's, as the original *punctum ſaliens*, whence all the miſchief aroſe; and though they in a ſummary mode ſay, it "was *evidently propagated* from one to another, and AFTERWARDS *conveyed to other neighbourhoods*," yet it would have been candid to ſtate to the public what appears to be well known to the medical gentlemen of Philadelphia, that numerous caſes of the diſeaſe appeared in parts of the city far diſtant from Criſman's, and in perſons who had never been near his terrible houſe. The facts will doubtleſs come to light in due time, but it may be well to mention to the board, the caſes of the men employed in laying the water logs in the weſtern parts of the city, and thoſe of others who died in the Pennſylvania hoſpital, in order to ſhew, that notwithſtanding their partial ſtatement, more of the whole truth has reached this remote

ſpot than they appear willing ſhould be known. No one pretends that the cauſes, though certainly domeſtic, operate like the Samiel or Sirocco blaſts, which rapidly deſtroy all within their ſphere. We only know from the hiſtory of former fevers, that though ſcattering caſes occur in all quarters of the city previouſly to the ſpread of the grand epidemic, (thus *ſhewing the operation of the general cauſe*,) yet that it appears with peculiar violence in a certain quarter, which by the people at large, who cannot know particulars, is ſuppoſed to be the ſpot to which the contagion was firſt brought.

The neglect of the people in attending to the wiſhes of the board to evacuate the neighbourhood, and to avoid intercourſe with the ſick, muſt not be admitted as a reaſon for the prevalence of the diſeaſe; nor does it follow, that, becauſe the diſeaſe at Providence was thus arreſted in its progreſs, the ſame effects would have followed a ſimilar conduct in Philadelphia; for the board take for granted that the ſame diſeaſe prevailed in both places, which is by no means proved; nay, it may be doubted whether, had the ſeparation of the well from the ſick been ever ſo completely effected, the reſult would have been ſuch as was predicted. The meaſure has never failed to arreſt the progreſs of the ſmall pox, meaſles, plague, jail, camp or hoſpital fever, and other diſeaſes TRULY contagious, but the attempt has uniformly failed in every inſtance of the American peſtilence in which it has been tried. Who does not recollect the proclamation of the governor of Pennſylvania, which he iſſued at the requeſt of the board of health during the fever of 1797, commanding the communication with the people of Penn-ſtreet to be cut off; and the ſuſpenſion of yellow flags from the windows of the ſick in that quarter*; and what was the reſult? The rapid appearance of the diſeaſe in other parts of the town, with apparently increaſed violence, ſhewed the weakneſs of the meaſure and the

* See Folwell's Hiſtory of the Fever.

univerſal nature of the cauſe. We even find that notwithſtanding the uſe of military cordons round the infected towns of Spain, the diſeaſe ſpread with unexampled violence through Andaluſia during the laſt year; the wretched inhabitants thus ſuffering from the combined effects of an ignorant adminiſtration, and an external enemy.

To conclude. If the doctrine be eſtabliſhed, which the board inculcate, that two boys can find their way down to the Lazaretto, barricaded and guarded as it is, and entering the room of people confined there with contagious diſeaſes,* bring back to the city the complaint they have taken, there can be no heſitation in declaring THAT THE WHOLE QUARANTINE SYSTEM IS ABSURD AND OUGHT TO BE ABOLISHED. The preſervation of the public health, would be ſome compenſation for the empty wharves of the Delaware, but the loſs of both commerce and health, are evils which no country, however rich in reſources, can ſupport.

MEDICUS.

* The actual viſit to the ſick, by the two boys in the preſent inſtance, however, remains to be proved. The fact is admitted for argument ſake.